SCIENCE, TECHNOLOGY, AND WARFARE

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SCIENCE, TECHNOLOGY, AND WARFARE

The Proceedings of the
Third Military History Symposium
United States Air Force Academy
8–9 May 1969

Edited by
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PREFACE

The theme of the first Military History Symposium, held at the United States Air Force Academy on 4–5 May 1967, was "Current Concepts in Military History." The papers delivered at the second, on 2–3 May 1968, together with the subsequent comments of a number of officers who had participated in the events discussed, were published by the Academy in 1969 as *Command and Commanders in Modern Warfare*. The present volume consists of the papers, revised and annotated for publication, and the discussion sessions of the third Symposium, held on 8–9 May 1969. With the fourth Symposium, "Soldiers and Statesmen; the Policy-Making Process in Modern History," to be held at the Academy on 22–23 Oct 1970, the series becomes biennial.

The Symposia are intended to serve a number of purposes. First, they provide a forum for scholars in military history, a field that has grown rapidly since World War II and one in which the Academy obviously has a special interest. Second, by bringing distinguished scholars to the Academy, the Symposia provide a link between the scholar and the military professional. At a time of serious internal stresses in American society, all such links are generally valuable. More prosaically, however, the Academy's history faculty is kept abreast of developments in their academic discipline, while cadets are encouraged to a continuing interest in the background of their chosen profession. Third, by the participation of historians who do not consider themselves primarily "military" historians, but who are competent in areas that impinge on military affairs, the field of military history itself is enriched.

The participants in the Symposium are identified in the

final section of this volume. The Department of History and the Association of Graduates, USAF Academy, thank them, once again, for their individual and collective labors. The commentators were requested generally to add to the basic papers with reference to their own areas of special competence, thus broadening the content of the total Symposium. This charter did not preclude scholarly criticism of the major papers, as the reader will discover for himself.

In addition to the participants, the Symposium required the combined efforts of a number of individuals and organizations. The active support of the Superintendent of the Academy, Lieutenant General Thomas S. Moorman, and of the Dean of the Faculty, Brigadier General William T. Woodyard; the warm encouragement of the Commandant of Cadets, Brigadier General Robin Olds; and the financial support of the Association of Graduates are acknowledged. Within the Department of History, the Symposium was truly a departmental project: everyone was involved, directly or indirectly, with the countless logistical details. The chief secretary, Miss Marjorie Burton, should be singled out for special mention, as should Mrs. Carolyn Stamm, who transcribed the tapes of the discussion sessions.

The 11th Harmon Memorial Lecture, *The War of Ideas; the United States Navy 1870–1890*, by Professor Elting E. Morison, Yale University, has been published separately. Because it was an integral part of the Symposium, it is reprinted here.

M. D. W.
September 1970

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INTRODUCTION

The nature of warfare has always been largely determined by contemporary technology. Instances of technological change undertaken for the sake of military advantage have also been relatively common in history. The relationships between science and warfare, however, have been much more variable and ambiguous. The papers and discussions of the Symposium investigate selected aspects of the complex relationships between science and technology on the one hand, and warfare on the other, from the Renaissance to the 1960s.

In the first session, Professor Hall takes up in turn the possible areas of interaction between science (exterior ballistics, engineering, explosives, mechanics, and metallurgy) and military technology (edge weapons, cannons and mortars, fortification and siege warfare, and small arms) in the 15th, 16th, and 17th centuries. The notion that science is pursued for utilitarian ends, Hall finds, is an "unhistorical projection backward from our own age." He excludes navigation and medicine from consideration, because they were civil as well as military concerns. In spite of the pleading of certain early propagandists of the "Empire of Man over Nature," and in spite of the elaborate sketches of military engines in Leonardo's notebooks, military technology was largely innocent of scientific method. The developments in fortification required mathematical skills, but nothing more than elementary geometry and arithmetic. Mathematicians studied the ancient problem of the trajectory of projectiles, but their efforts affected neither the design nor the use of guns. The range tables they provided were not even usable with the guns of the time. The solution of the trajectory problem would await Benjamin Robins and the 18th century.

Professor Hale supports Hall's conclusion with three arguments. In the 16th and 17th centuries, armies were so organized as to preclude any productive contact with the worlds of science and technology. Money was lacking for gun foundries to test new weapons in peacetime, so that when war began, existing models were put back into production. Second, the soldier's status had never been lower. To refurbish the image, literate soldiers devoted their efforts largely to recalling the traditional values of discipline and morale and gave little thought

to new weapons. While they made much of the need for liberally educated officers and published elaborate diagrams of infantry formations, such writings were almost entirely image-building and had no practical effect on what actually happened on the battlefields. Finally, soldiers had no understanding of what science was and therefore could not call on its aid. The idea of progress was not widespread, and technological advances were isolated, not appearing to the soldier as a process of which he could take advantage.

Professor Wolf emphasizes the military consequences of certain technological developments. While scientists contributed little or nothing, craftsmen, proceeding by trial, error, and accident, developed the cast-iron gun which was cheap enough to be produced in large quantities; and effective artillery changed the face of war. The development of a reliable fusil, particularly after the addition of the bayonet, also had profound effects. New portable bake ovens, pontoons, trenching tools, copper-sheathed hulls, improved sail plans, all had military potential. Tactical changes to take advantage of technological advances were generally slow. The most important development in warfare was the advent of the bureaucracies that controlled armies and navies. As they became stronger, administrators were able to insist on accepting technological changes, as well as other reforms.

In the discussion, Professors Ropp and White develop Wolf's last idea further, suggesting that the Symposium's topic, "Science, Technology, and Warfare," required a fourth term to be complete—Management—because the primary military innovator never has been the scientist, technologist, or soldier, but rather the administrative "organizer of victory." White also notes that Hall's remarks concerning soldiers garnishing themselves with the ornaments of borrowed science, for the sake of status rather than efficiency, may apply into the 20th century.

Mathematics is basic to a technical education. In 1751, Louis XV established the *École militaire,* and its curriculum was heavy in mathematics. In the second session, Professor Bien explores the reasons for this choice and finds that they were not based on any analysis of the officer's job, because most graduates of the school became infantry and cavalry officers and needed only simple arithmetic, not geometry and algebra. Rather, Bien finds that the math-centered curriculum was justified as producing officers who could think clearly and reach sound conclusions. Educational reformers were concurrently remodeling the humanist curriculum, strengthening the teaching of rhetoric so that it would emphasize order, clarity, and precision of thought. Rhetoric might, therefore, have served the military school, but rhetoric was suspect to the military reformers, as were all things of the literary

world; for the literary world was inhabited by egoists, by men who would not obey orders, by those who resisted the taxes that would permit overhaul of the army, and by those who looked to the army as a place where they could buy commissions for their sons. The decision to center the curriculum on math was part of a larger attempt to seal off the military from civilian culture of the time.

Professor Shy notes that, while military education in 18th century France was not dictated by technological pressures, the technological plateau on which European armies operated up to the 1840s permitted military pedagogues to tailor their schools to serve non-technological purposes. The *École militaire's* emphasis on mathematics was peculiar to France. Frederick the Great's military schools emphasized a literary education, much like that disavowed by the French. The French Revolution and Napoleonic Wars were fought mostly by officers other than graduates of the *École militaire,* but the French military schools of the Restoration maintained the earlier emphasis on mathematics. And with the founding of West Point, Sandhurst, and the reformed General War School at Berlin, all within a short period around 1800, the emphasis on mathematics passed beyond the French military schools. The long-term effects of this emphasis are unclear. Officers were better prepared to appreciate technological changes when they did occur; but even more, they may have come to see their profession as "essentially geometric and algebraic in character," just as the early French reformers had hoped.

Professor Hughes does not rebut Bien's thesis but argues that the relationship between technology and warfare generally, and the importance of mathematics in the dominant mode of warfare, siegecraft, specifically, together with the increasing role of artillery required a math-centered curriculum. The relationship is so obvious to Hughes that he believes "it would be more difficult to explain a failure to stress mathematics than to explain the stress on it."

Professor Rothenberg surveys military education in the 18th century Austrian army. The government required that line officers only be loyal and brave. Force of circumstance, however, caused the army to educate officers for technical service with the engineers and artillery. The specialist schools were supported largely by private individuals, and the government remained suspicious of technically trained officers, because of the social implications of scientific education.

In the third session, Professor Holley investigates the United States Air Force's use of operations research. The British and American pioneers had made believers of wartime air commanders, but institutionalizing operations research in the peacetime service proved

difficult. While wartime researchers had been drawn from diverse academic specialties, the postwar organization was staffed almost exclusively with mathematicians, scientists, and engineers, a change advocated by the burgeoning Operations Research Society of America. The peacetime operations research office in USAF Headquarters was for several years located far down the chain of command and lacked control over similar offices at the headquarters of major commands (SAC, TAC, etc.). Much important research, particularly on questions of broad significance to national security and long-range planning, was referred to an external organization, RAND. In time, RAND reports encouraged similar work within the Air Force, if only to blunt a RAND proposal with a counter-proposal, based on equally impressive research.

In 1959, the status of operations research at USAF Headquarters jumped dramatically and the possibilities of reorganizing Air Force operations research, making the various offices more responsive to central control, were investigated, but no significant reorganization occurred. With the advent of Secretary of Defense McNamara, operations research began to be pushed into new and larger problems, questions that increasingly resisted quantification. The larger questions required different techniques, which received a different name: systems analysis. Many researchers resisted the change. As the need for such analysis increased, a new Studies and Analysis Office was created. In 1967 the new Secretary of the Air Force ordered a review of the entire operations research effort. On the basis of the independent review, the Director's position was strengthened relative to the various operations research offices throughout the Air Force. A new operations research office at Headquarters, 7th Air Force, Saigon, as well as several ad hoc groups, took up a number of pressing problems stemming from the combat in Southeast Asia. In several cases, their achievements have rivaled those of World War II.

Professor Holley concludes that the Air Force has made less than optimum use of the tool since World War II, because of an ineffective structure, or an inadequate doctrine, for the application of operations research.

Mr. Perry finds Holley too charitable. Perry notes that the main achievements of operations research in the RAF were made by engineers at a time when Great Britain was losing the war. Desperate measures were called for, and the British overcame the traditional military distrust of science and scientists to take advantage of their contributions. Operations research of both the British and Americans during World War II dealt mostly with routine procedures, things that had to be done, were being done, and might be done more efficiently. The quantum jump implied in the postwar change of name

to systems analysis not only required dealing with many unknowns: more significantly, it required asking questions of vast, future significance. When the Air Force was unwilling or unable to choose between competing weapon systems, the Secretary of Defense intruded into areas that had formerly been reserved to the military. The Air Force did recognize the value of operations research and institutionalized it, so that it would be available for the next war. But meanwhile, as a part of the institution, operations researchers were as incapable as any bureaucratic group of fundamentally reforming their own bureaucracy; and whether the individuals in the bureaucracy were scientists or humanists was immaterial.

In the second major paper of the third session, Professor Kranzberg argues that the "interaction between science-technology and warfare is quantitatively greater in the post-World War II era than ever before in history and qualitatively different." Science-technology and the military were closely connected in Jefferson's day. West Point was founded as much for civil as military purposes, but the Military Academy became estranged from American society after the Civil War. In both the Civil War and World War I, scientific organizations were formed to aid the war effort; but between the two World Wars, the ties between scientists and the military almost disappeared. Their collaboration during World War II was unprecedented, remarkably productive, and relatively free of dissension.

The rapid technological changes since World War II, in energy sources, materials, transportation, automation, qualify as a revolution; and warfare has shared in the changes. The more exotic weapon systems depend on the successful application of a number of sciences and technologies. Military management has been revolutionized, in response to the complexity and cost of the new technology, as well as awareness of social considerations. In the university, the disciplinary boundaries that separated the various scientific and engineering fields have become indistinct; in the Pentagon, the armed forces have lost their unique missions. The non-profit institutions have transformed the nature and direction of scientific and technological activity, and the military has led in this institutionalization of research and development. Military technology ended America's free and absolute security, and the military must now rely increasingly on technology for conditional, and very expensive, security. Our initial postwar strategy rested simply on the atomic bomb monopoly. Since then, strategy has become increasingly complicated, seeking to meet a spectrum of threats. Not only at the upper end of that spectrum has technology been applied to warfare. Weaponry for limited, conventional war has become sophisticated. The most important external pressure operating on

military strategy, and hence on science-technology, has been Russian and Chinese capability.

Kranzberg finds the chicken-and-egg and pendulum analogies of no help in understanding the relationship between science-technology and warfare. The stereotyped theory of a linear relationship between science, technology, and warfare satisfies the facts in only a few instances. Much more satisfactory is a push-pull model. A scientific discovery may attract ("pull") military attention, which then demands ("pushes") technological development of that discovery into a weapon. A scientific or technological advance in a non-military field may be seen by the military as of potential use, if adapted via further technological work. In the process, it may be found that additional, basic scientific experimentation is needed. The military may also push for undirected scientific research, hoping for future application in a completely unanticipated way. Science-technology breakthroughs have historically accounted for most of the major advances in military strategy and weapons; but military requirements have frequently pushed successfully for specific and technological advances that, in the aggregate, are impressive. Both fundamental and applied research play essential roles in the innovative process, but the applied becomes increasingly important as the time of the final innovation approaches. Both heavier-than-air flight and rocket missilery were the products of civilian investigations; both were rapidly developed through the military push. The military push has sometimes demanded more than science and technology could deliver: witness the TFX and M–14.

Military technology differs markedly from civilian technology today, with only slight direct adoption from one to the other. Today's military requirements are too specialized and change too rapidly. Defense industry must work under great pressure and accept great uncertainties. Cost is not the prime factor in military technology, and many of the exotic weapons are hand crafted. Defense industries have a high percentage of scientists, and this offers one new means by which the two technologies interact: military technology trains engineers who then find employment in the civil sector. The permanent defense industry came into being with the Cold War and the Korean War, and the resulting military-industrial complex has reversed some trends in American society.

Because of American worldwide commitments since the advent of the Cold War, because the American government has entered new areas of activity since the New Deal, and because the military occupies a place of increased importance in government, the military is deeply involved in a number of new areas: desegregation, civil order, economic growth, education, research. And much of the importance of the

military, Kranzberg believes, stems from its closer relationship with science-technology.

Professor Lasby suggests that the alliance between science, technology, and warfare has never been as easily consummated as appearances indicate and believes the reasons are to be found in the whole, broad context in which events occur—including personal interests, emotions, external events, and accident. Operations researchers feared the loss of their identity if they broadened their efforts into areas for which their skills seemed inadequate. A similar situation occurred in the attempt to import large numbers of German scientists and engineers into the United States immediately after World War II. Although the need for their skills became apparent in the opening months of the Cold War, the program was never as successful as its supporters had hoped because of opposition by individuals and groups to the importation of former Nazis into the Western Hemisphere. Lasby believes that the intrusion of such peripheral issues still helps explain the friction apparent in the working of the military-industrial complex.

In the discussion session, Professor Brodie suggests that systems analysis, under McNamara, became "unduly prestigious" and its practitioners gave advice in areas of national defense policy where neither systems analysis nor the analysts were competent. Dr. Fisher predicts that the rapid growth of technology, so far characteristic of the 20th century, cannot continue but will level off, and the technological revolution will end, because of the ever-increasing costs. At that time, warfare will again become more of an art, relying less on continual interaction with science and technology, although military technology will be much more complex than today's. Colonel Kane takes issue with Holley's insistence on the importance of organization and doctrine, believing individuals are more critical in the success or failure of analysis. Kane supports Holley and Perry with observations confirming the Parkinsonism inherent in institutionalized operations research. Kane also asks for a greater use of history in the formulation of strategy. Dr. Emme criticizes the penchant of many historians for model building and case studies—such as those featured in the third session—believing such methods are insufficiently broad to achieve "solid history." Such history should be written on many topics in contemporary military technology, if future historians are to be able to avoid perpetuating myths.

Professor Morison, in his Harmon Memorial Lecture, used the post-Civil War United States Navy as a departure point from which to approach contemporary problems. The Navy had systematically turned its back on new ideas and weapons following the Civil War, to return to an earlier era, one that was more comfortable for many of the

officers, but one in which habit and tradition held almost undisputed sway. There was no general, ruling idea as to the purpose of a navy, so that when new ideas appeared, they could not be related to an overall scheme and were therefore discarded piecemeal. Only with Mahan's theory did this condition change. Morison then considers the opposite problem today: one in which the society, including the military, is conditioned to rapid and continual change; and he suggests that this may be placing too heavy a load on the military, which is after all an institution that requires a certain amount of dedication to routine, to faith and tradition, if it is to function. Further, the modern enthusiasm for concentration on means, for rapid technological improvements, may result in insufficient attention to ends. In the overriding concern with hardware, more attractive alternative policies may not even be considered. Other institutions have been overloaded—the cities, universities, established habits and conventions. What is needed is a new general, governing idea, such as Mahan's, an idea that can lead to new institutions and new values that "will enable us to control the extraordinary energies and applications that we have power over, in such a way that they will serve man and society most effectively." Morison sees such cooperative ventures as the Symposium as a promising beginning in that quest.

The First Session

SCIENCE, TECHNOLOGY, AND WARFARE, 1400-1700

SCIENCE, TECHNOLOGY, AND WARFARE, 1400-1700

A. Rupert Hall*
Imperial College of Science and Technology, London

No historian ought to have the impudence to speak in public on the topic I have chosen for today; certainly no historian can do justice to it without a polymathic ability which I myself, as a historian of science having only a nodding acquaintance with the evolution of technology, do not possess. As for the history of war, I have followed it only in desultory fashion. But at least I have examined one aspect of this multifarious problem in some detail—and not everyone who has had ideas about the historical relations of science and war has done even that.

It has been and I suppose still is commonly assumed that there is a casual if not a causal connection between the growth of science and the development of military techniques: weapons use explosives, and these are products of chemical science; navies navigate, and navigation is a special kind of applied astronomy; weapons have been improved through metallurgical skills, which themselves arise from science; and the engineer who employs a lot of difficult formulae printed in a little book is also a sort of mathematician. These crude correlations enable people to argue either that scientific research has been pursued for the sake of military advantage, or that soldiers have always been successful in harnessing scientific philosophy to their chariots. Without war we would have had a more primitive science, without science a more primitive art of war. Now I would not deny that waging war and preparing for war have been most potent agencies in the formation of our civilization, nor that rewards dazzling the eye of the inventor (though ever so rarely won) have caused virtually every piece of human knowledge to be exploited in war, at least in imagination. Nevertheless, the attitude I have described—which we may think of as extremely ancient, since it compelled the Romans to misunderstand

*Professor Hall was unable to attend the Symposium because of illness. In his absence, the paper was read by Professor Lynn White, Jr.

the true character of Archimedes—is very largely a piece of wishful thinking. It happens to have a measure of justification for our own age, but no earlier one. Rather over a century ago the Reverend Francis Bashforth, who ought, one might suppose, to have had his mind on even higher and swifter things, devised an electric chronograph for measuring the speed of projectiles, whereby ballistics ceased to be a speculative branch of applied mathematics and became an applied science. With that event, to my mind, the science of war begins to replace the art of war. The era in which a few men in white coats can, with a year or two's work, determine the fate of battalions, ships, or airplanes had begun. But not before then.

Throughout the period with which I am concerned today military affairs were determined by three things: organising ability, including what we today call logistics; basic craft skill; and courage, which is perhaps some sort of social attribute. They no more depended on scientific knowledge than they did on the relative size of the combatants' populations or their real economic strength—think of the wars of France and Holland in the seventeenth century. If you make any study of the competent military commanders of the sixteenth and seventeenth centuries, you will find that they insisted on proper equipment and supply (so far as was possible in those days), that they trained their men in use of weapons and military evolutions, and that they sought to inspire them with courage; you will find no evidence that commanders believed in science or mathematics, or expected any other skill in their troops than that of managing their pieces dextrously and carrying out orders on the field. Perhaps a critic may object: but what of gunnery and ballistics, what of fortification and geometry? I will deal with these points later.

It seems to me that the notion of war's indebtedness to science, and indeed of the pursuit of science for utilitarian ends, is a very unhistorical projection backward from our own age, in which warfare has indeed been transformed by science, countenanced by reference to special pleading on the part of some early propagandists of modern science. I do not by any means deny that Francis Bacon wrote of the "Empire of Man over Nature" and so forth, or that a few other apologists, in the search for public acclaim and finance, boasted of the practical good that science would do, especially in its applications to agriculture and medicine. I do deny that these propaganda claims affected what the natural philosophers, mathematicians, and scientific societies actually concerned themselves with. The argument that inventive craftsmen, with their special concern for promoting the "Empire of Man over Nature" by rational and experimental investigations, formed the spearhead of the scientific revolution of the seven-

teenth century has in my opinion rested on a very dubious use of the historical evidence and indeed downright errors from the time of Edgar Zilsel to that of Christopher Hill.

Before I can proceed to the limited section of this historiographical debate that is relevant now, I must propose some distinctions and definitions. For without some rigour in this respect anything is demonstrable. I shall define science as a rational and theoretical inquiry into nature, having as its objects description, analysis, and explanation. Technology is the practical knowledge and skill by which material ends are accomplished. Science and technology are often associated. A man can be a chemist and a skilled glass-blower, a mathematician and an airplane pilot. Science may be required for the design of a certain instrument, technology for its actual fabrication. But we should not confuse things, when the use of terms is critical, by speaking of astronomy as a craft, or surveying as a science. Thirdly, we should distinguish between the case where the technologist applies, for his own purposes, existing scientific knowledge and the case where a scientist tries to solve a problem in order to present its solution to technology. As examples of the former one might cite the development of scientific navigation and cartography by the Portuguese in the fifteenth century, and the introduction of the gunner's quadrant in the sixteenth. This quadrant in its various forms was an inclinometer. No doubt its ultimate parent was the astrolabe which brought forth such a large progeny, and it appears in the *Trattati* of Francesco di Giorgio Martini (1439–1501), written in the late fifteenth century. In my own view this famous gunner's quadrant was never much more than an elaborate piece of mystification intended to enhance craft prestige, but let that pass. As examples of the scientific search for the technically useful solution to a problem, it is commonplace to indicate the interest of seventeenth century astronomers in the determination of longitude, associated with the foundation of the observatories of Paris (1666) and Greenwich (1675). When he has made such a distinction clear the historian may also appreciate the significance of a second one arising from it: the fact that he builds a technique upon science does not make the craftsman or technologist a scientist. The navigator with his backstaff, the gunner with his quadrant and range table, is not an astronomer nor a mathematician. Conversely the scientist who dreams (shall we say) of smelting iron with coal instead of charcoal does not thereby acquire the practical skill of an ironmaster. Even taking out a patent will not make him a craftsman. As for the inventor, he is only too often rightly to be dismissed as a "projector," a dreamer lacking both philosophic reason and craft experience.

Now let me try to isolate more particularly the possible areas of

interaction between science and war in the renaissance age. First, two exclusions. I shall not consider navigation as it affected naval operations, for the simple reason that though indeed navies generally took more trouble over navigation and cartography than the merchant marine, government service has never furnished the sole opportunity for the exercise of navigational skills. For instance, when Galileo offered his new-made (not invented!) telescope to the Signoria of Venice, he was clearly thinking of Venetian commerce, not her defence. For similar reasons and lack of time I say nothing of medical science and war. It is a commonly held view that pressures of war have stimulated advances in medicine, and in the sixteenth century one could point to the career of Ambroise Paré as exemplifying this; but equally civil life presented enormous problems of which all were aware, for example in this period syphilis and plague, to which great medical endeavours were devoted. These matters set aside, and disregarding such obvious trivialities as the use of arithmetic in ordering troops into formation and the computation of logistic needs, we are left with these possible areas of interest:

TABLE 1.—MILITARY TECHNOLOGY

	Edge Weapons	*Cannon and Mortars*	*Fortification and Siege Warfare*	*Small Arms*
Ballistics (ext.)	—	X	X	—[1]
Engineering[2]	—	—	X	—
Explosives	—	X	X	X
Mechanics	—	X[3]	X[4]	X
Metallurgy	X	X	—	X

[1] No one at this time thought of using calibrated sights or other complex aids to long range shooting on small arms.

[2] I use the word here in the older, more restricted sense, including everything from the design of fortifications and siege works to the moving of earth and construction of masonry, but excluding mechanical engineering.

[3] This includes cranes and hoisting devices, boring machines, carriages and limbers, and much paraphernalia needed for the manipulation of heavy guns.

[4] This includes both mechanical devices used in engineering and those employed by besiegers against a fortification.

I shall deal with weapons first, remembering always that close-range weapons are the decisive ones in this period: sword and pike, pistol and musket, fieldpiece or small naval gun. It must strike us at once that the all-important metallurgical knowledge and craft skill by which weapons were fabricated throughout this period, and long after, were quite unrelieved by any light from science. The armourer forged, formed, finished, and ornamented steels for helmet and cuirass; the bladesmith carefully compounded soft and hard steels to produce cutting, flexible

swords; the locksmith worked with file and chisel on half a dozen curly little levers and so on. All this was totally innocent of chemical science and crystallography. Locks, barrels, blades were made by specialised developments of the common blacksmith's skill. The gunfounder, again, excelled in his art because he prepared larger and often more beautiful castings in bronze or iron than any other metal-founder; but he too knew nothing of metallurgy, not so much as the nature of the difference between cast iron, wrought iron, and steel. Of course, I do not mean to say there was no metallurgical invention; invention of bellows and furnaces, fresh alloys of bronze, and even treatment of iron castings went on all the time. But none of it was invention based on science.

Consider now the mechanics of weapons. As my diagram showed, this question is relevant to small arms and siege warfare. I think it would be pedantic to go into such details as the mechanical design of gun carriages, based on conventional waggon practice, or such a will-of-the-wisp (first found in the fourteenth century) as the "armoured car"; it was a favourite of the fifteenth century engineers, and Leonardo's drawing, so perfunctory as to be impossible, is familiar. Siege engines were always a speciality of war. They too existed, or I should rather say were illustrated, in Leonardo's notebooks and those of Francesco di Giorgio Martini, as well as the printed work of Roberto Valburio (1472). I have no evidence that such cumbersome, complex machines

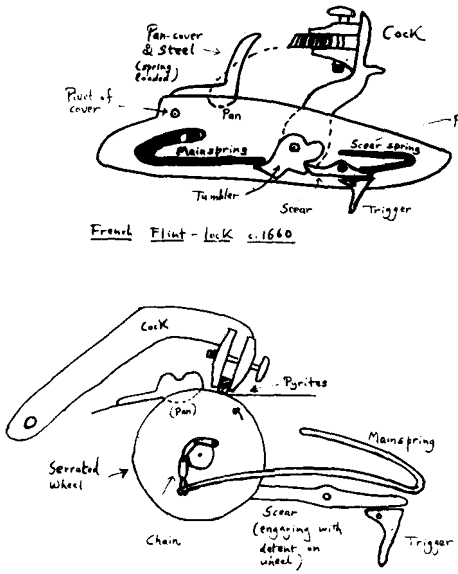

Fig. 1. Lock Mechanisms.

were actually employed in war at this time; again, I think you will agree that their construction presented no unusual problems, save that of scale perhaps.

Far more interesting, and really important, is the history of the small arms lock mechanism. You will recall that three main devices appear in overlapping succession: the matchlock, the wheel lock, and the flintlock, the last showing many variant forms. The objects constantly in view through the evolution of the weapon were reliability, especially with respect to damp, and convenience of recocking. The matchlock was cheap, simple, unreliable, dangerous, and tedious. The wheel lock was a beautiful, expensive, and reliable device, slow to recock. Therefore it did not wholly replace the matchlock. The flintlock was good in every way, also fairly cheap and robust; therefore it remained standard for a century and a half. So far as I am aware we know nothing of the first inventors of these devices, all in the sixteenth century, still less of the origin of the local variants. But I have never seen it suggested that the inventors were not working gunsmiths, presumably masters of the locksmith's craft. I find it extraordinarily difficult to see how science or mathematics could have had any part in this evolution of small arms.

Casting around a little more in the same area of technology, we discover that hunting weapons were rifled from the early sixteenth century, that breech-loading mechanisms were introduced in such luxurious weapons,[1] and that the multi-shot idea was applied in a variety of ways (one of the simplest, of course, being the revolving cylinder).[2] All these inventions, and there are many of them, are related by experts on firearms history to the craft, and it is worth noting that this adventurous technology, never (except for rifling) really successful, was devoted to expensive, personal weapons that are beautifully ornamented, not to the common soldier's musket. The reasons are obvious. And with respect to rifling, observe that it was the rich sporting marksman who was interested in accuracy; the tactical practice was such that accurate individual fire at aimed targets had no place on the field of battle, at any rate before the War of Independence.

The only gift of science to small arms technology in this period was

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[1] As one example among many, the Marquis of Worcester's patent of 15 November 1661 refers to an "invention to make certain guns or pistols, which in the tenth part of one minute of an hour may . . . be recharged, the fourth part of one turn of the barrel (which remains still fixed) fastening it as forceably and effectually as a dozen threads of any screw . . . ." Clearly this is a multi-thread, quick-closing device for a breech block.

[2] Again, one example: Pepys refers on 3 July 1662 to "a gun to discharge seven times, the best of all devices that ever I saw, and very serviceable and not a bauble"; similarly on 4 March 1664.

the air gun. Scientific interest in pneumatics through the mid-seventeenth century revealed the exceptional force not only of atmospheric pressure (von Guericke's "Magdeburg experiments," 1654) but of compressed air also, and provided the pump technology. The rest was easy. But the "philosophic" gun was never more than a curiosity, or an assassin's tool (as in Conan Doyle's story) except when it was adopted in the Austrian army in 1780. This 30-shot rifled sharpshooter's weapon was no toy, since its highest ball velocity approached 1,000 ft/sec, and the calculated energy was 400 ft/lbs, but it was far too complex and costly for ordinary use.[3]

We can almost as briefly dismiss explosives. There was a literature of gunpowder-making at least from 1420, when the tradition of the *Feuerwerkbuchen* began; this was first printed in 1529, and Biringuccio also dealt with powder in 1540.[4] There was a constant search for the means *"Wye man noch eyn besser und stercker Pulver machen soll,"* resulting in scores of recipes, whereby not only were the proportions of the normal ingredients varied, or the ways of preparing them (especially the charcoal) modified, but other irrelevant ones added, like camphor, sal ammoniac, sublimate of mercury, vitriol, or brandy. An important step was introduced towards the end of the sixteenth century when powder was "corned" or granulated, instead of leaving it in a fine, floury state. Early in the seventeenth century a new, very touchy explosive was discovered: fulminating gold. This remained a chemical curiosity of no destructive value. By Louis XIV's reign a considerable industry was devoted to recovering saltpetre from the efflorescence on walls and nitrogenous animal wastes, to obtaining sulphur from volcanic and other deposits, and to the milling of these ingredients, just as there was a considerable art devoted to preparing various grades of powder as needed for blasting, cannon, small arms, or fireworks. But there was no chemical science in this. Philosophy speculated about volumetric changes manifest in the passage from fluid to "air" (steam) or from solid to "air" (powder fumes). Philosophers realized that the force of powder sprang from the expansion produced by heat, and even endeavoured (from 1675) to capture this force in a piston-and-cylinder arrangement. Some philosophers—not all—accepted it as a fact that gunpowder could burn in the absence of air, and argued from this that saltpetre and air possessed in common a "something" necessary to fire; a few imagined fire to be a reaction between "sulphureous," that is combustible, particles and "nitroaerial" particles. However, all this fascinating speculation was of no value to the gunpowder-men.

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[3] F. H. Baer, "The Air Rifle That Went to War," *American Rifleman* 115 (Dec. 1967): 32–35.

[4] Wilhelm Hassenstein, *Das Feuerwerkbuch von 1420* (Munich, 1941).

It would be foolish to deny that the medieval invention of gunpowder furnished the philosophers with a fascinating subject for speculation, but I know of nothing that would indicate gunpowder's making any major contribution to the evolution of chemical science, or vice versa. Perhaps I should mention that in both the early Royal Society and the Académie des Sciences there was some interest in little devices by which the quality of samples of powder might be tested; but this, and one or two like points that cropped up, seems awfully trivial.

I propose to leave gunnery and mathematical science to the end of my talk, turning now to the technology of static defence and attack. First mechanical engineering. There were two ways in which machines could be of great assistance: they were useful in earth-moving for defence, and in providing cover for attack. Leonardo shows us a sketch of a great ditch-digging machine, for use in swampy ground particularly; Ramelli pictures a soil-conveyor replacing the more usual ramp-and-barrow. Obviously these machines were as applicable to civil engineering as to military; recall that there was very considerable activity in canal building during the sixteenth and seventeenth centuries. However, authors and inventors seem as regularly to have associated earth-moving with fortification, as they associated dredging and pile-driving with peaceful commerce. And there is this curious difference too. We know that the dredges and pile-drivers really existed, for there are accounts of such machines in action. But I am not aware that excavating machines or conveyors were used at all in practice (unless we call the tram-road and truck, as in Agricola, a conveyor). I believe we will not go far wrong in supposing that pick, shovel, and barrow were the appliances really in use—the ones that built our canals and railways.

Earth and stone for defence; timber obviously for protection of the offensive. The permutations of a few simple technical ideas extending back to remote antiquity were endless, from Leonardo's promises of portable bridges and armoured cars, scaling ladders, mantlets, "and other instruments" in his letter to Ludovico Sforza, to the Marquis of Worcester's invention of a "transmittible Gallery over any Ditch or Breach in a Town-wall, with a Blinde and Parapit Cannon-proof." Some devices of this sort were quite complex, as may be seen in the late-fifteenth century sketches of Francesco di Giorgio Martini. Were these ever really used? That they were in modern times seems unlikely, though they certainly had been in antiquity. In any event, there is nothing very involved about their construction.

With fortification proper we enter a very different world, and one that could devour the revenues of even a great prince. The site had to be carefully surveyed, and then the fortifying walls and towers properly

sited in order to protect the desired area; sometimes areas of housing had to be knocked down if they stood in the way of the defences, or impeded their usefulness. Expert engineers might differ as to the principles to be followed, as happened with regard to Berwick-on-Tweed in Elizabeth's reign.[5] But whatever the difficulties of application to a particular site, and these were grave, the military engineer throughout this period followed fairly clear principles—the ones that evolved first in Italy, in reaction to the introduction of cannon and the failure of medieval methods of static defence. The medieval castle repelled besiegers by the height of its stout wall, strengthened by round towers at intervals, and further protected by a moat which also impeded attempts to batter the walls or mine beneath them. Defensive missiles were hurled directly down from above. After the introduction of firearms a breach could be opened by cannon placed at a relatively safe distance from the wall, and the defenders could be annoyed by explosive bombs flung from mortars (this type of bombardment was familiar to both Valturio and Leonardo in the fifteenth century). If an attack was made on a breach, or the besiegers attempted a direct assault by scaling the wall, it was difficult for the defenders to concentrate their fire against them, harassed in turn by the counter fire of the besiegers. The first step the Italian engineers took in strengthening the defence was to break up the straight or curtain

[5] Lynn White, Jr., "Jacopo Aconcio as an Engineer," *American Historical Review* 72 (Jan. 1967): 425–44.

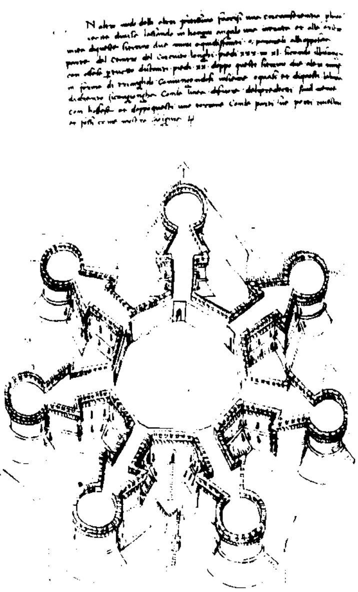

Fig. 2. Fortification Sketch by Francesco di Giorgio Martini.

wall between the towers, the weak spot, into a series of strong points or bastions, so that the whole circumference of the city bristled like a hedgehog. Here is a rather elaborate plan by Francesco di Giorgio Martini, also near the end of the fifteenth century. His bastions are imperfect, in that the bases of the round towers are not well covered by enfilading fire from other points. Enemy troops trying to get between the bastions would, however, be terribly exposed to a crossfire.

Defending troops using small arms were protected by parapets. Defensive cannon, both for counter-battery work and the immediate protection of the walls, were installed in strong casemates such as are still visible in the Elizabethan fortifications of Berwick.

In Bonaiuto Lorini's work on fortification (first published in 1592, but I have used the edition of 1609), the new principles fully elaborated in Italy by the middle of the sixteenth century, and derived from elementary geometry, are elaborately treated. Geometry, says Lorini, is not merely useful but essential, the very foundation of all our procedures. Without it and proper rules of proportion it would be impossible to construct a symmetrical, polygonal fortress. The engineer had to learn to set out the angles, slopes, and sides exactly to obtain the required protection.

To reduce the vulnerability of the walls to round shot, they were decreased in height and increased in thickness, earth replacing masonry at the upper level. To prevent escalading, the ditch was made much more formidable. Lorini's sectional illustration gives the general picture: note the sharpshooter at the end of the mined gallery.

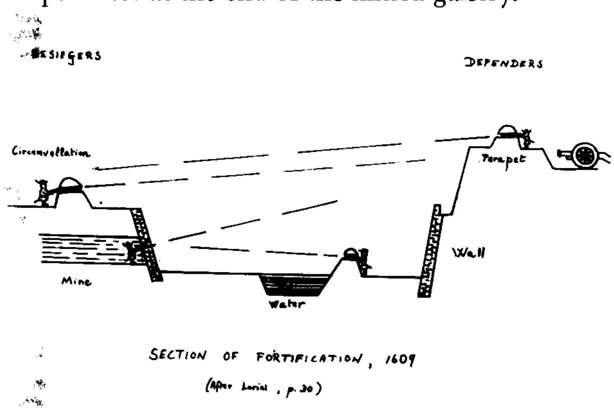

Fig. 3. Section of Fortification, 1609.

A great advantage of the new system, as regards the defender's firepower, was that it permitted flank fire by which not only the short curtain walls but the bastions themselves were protected against assault.

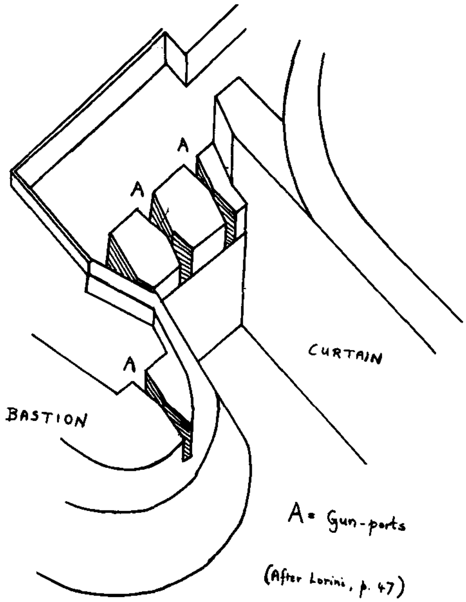

Fig. 4. Bastion and Curtain.

Lorini's drawing shows how the embrasures for cannon might be designed in order to provide this enfilading fire. Note that the guns at the root of the bastion, by being set back, are protected from round shot by the whole thickness of the bastion. It became the chief concern of the later military engineers, among whom Vauban was outstanding on the French side and Coehoorn on the Dutch, so to plot the angles of the perimeters of wall and bastion, together with the various supplementary ravelins, horn-works and so on, whose names were so loved by Uncle Toby, and the position of the defensive firepower on each, that the maximum amount of crossfire was directed at any point of attack. However, as may be seen from this rather detailed military scene published in a book of fortification in 1696, neither the attack nor the defence underwent further radical changes in the seventeenth century, though of course what is shown here is crude and out of date for the time.[6]

The question may now be asked: did the development of fortification in this period increase the pressure on the engineer's mathematical skill? I think to some extent it did. The problem of working out the

[6] Sebastien Fernandez de Medrano, *Ingenieur pratique ou Architecture militaire et moderne* (Brussels, 1696). General Medrano was "Directeur de l'Académie royale et militaire des Pays-Bas."

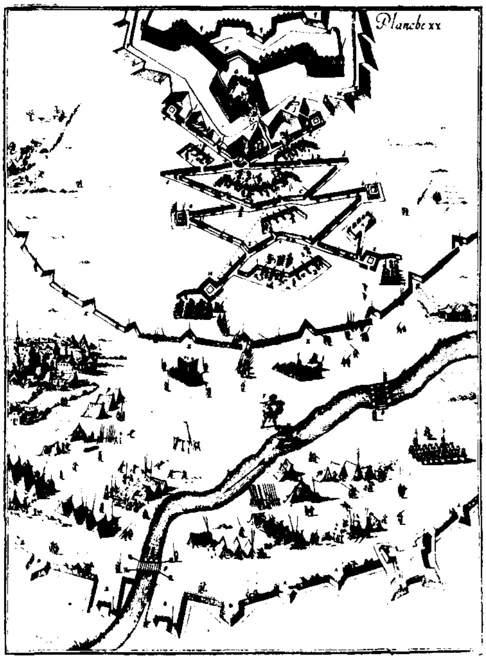

Fig. 5. Late Seventeenth Century Fortification.

form of a late seventeenth-century fortified city, with its elaborate rings of defence in considerable depth, multitude of planes, and countless firepoints was far more complex than making a plan for a medieval castle or a renaissance fortress. But not incomparably more difficult. Medieval and renaissance architects, we know, prepared designs of their buildings and employed geometry; we can follow the development of their tradition from Villard de Honnecourt (c.1250) onward. All renaissance architects and engineers claimed that geometry was the foundation of their work, that without geometry no building could be completed. Both Filarete and Francesco di Giorgio Martini were familiar with the Vitruvian notion that perfect architectural proportion was geometrically derived from the proportions of the human body. According to the former the mathematics can be learned from Euclid and Campano da Novaro (13th century).[7] Or as the poet Lydgate wrote: "by craft of Euclid mason doth his cure." Geometry was indeed the original secret of the freemasons.[8] It was very elementary; the skilled

[7] John R. Spencer, ed., *Filarete's Treatise on Architecture* (New Haven and London, 1965), 1:9.

[8] When this secret became devalued, the lodges in the seventeenth century became cult-centres.

military engineer of the seventeenth century needed considerably more mathematical sophistication than that cherished in the masons' lodges. Even so, no competent mathematician of the time regarded what was needed for engineering as other than trivial geometry and arithmetic, and it is almost ludicrous to see here an application of science as though something abstruse were going on. One might as well say that the literate engineer "applied literature" to his work. What we should see is a change, resulting ultimately from the introduction of artillery, in the attitudes and training of the architect-engineer, pushing him further along the road he had long before commenced to tread. I do not think the mathematicians had much to do with spurring him on; it was necessity that did so.

My final conclusion is this: the profession of the architect-engineer embraced the most highly sophisticated technology existing in the fourteenth, fifteenth, and sixteenth centuries; it was the one technical profession making large demands on organising and planning ability, drawing-office skill, taste, craft knowledge, and mathematical learning. We know from the lives and writings of Alberti, Filarete, Francesco di Giorgio Martini, Leonardo da Vinci, and others that the architect-engineer practised the arts of war as well as those of peace. We know that his had long been a proud, independent profession, only rarely willing to admit in some abstract speculations the superior ability of the academic mathematician, and in my view this situation changed little through the Renaissance. It was the architect-engineer who saw what cannon did to the old style of fortification and it was he who devised a new one—turning as always to his ancient sources of inspiration and strength, geometry and the laws of proportion.

The gunner, villain of the last few minutes, becomes hero of the next. He was socially, intellectually, and educationally the inferior of the architect-engineer. The books written by real gunners, like Robert Norton, Master-gunner of the Tower of London, are poor, dull, derivative books. The best books on artillery were written by gentlemen and generals, though perhaps they are not the truest. Perhaps I should make exception for one who saw service as a gunner (at Tilbury and Gravesend) in Elizabeth's reign, William Bourne, and who wrote about artillery (*The Arte of Shooting in Great Ordnaunce,* London, 1587) as well as other usual topics of the mathematical practitioner. But even he is by no means one of the great exponents of the art.

What is surprising about gunnery is that the mathematicians took it up—not of course the really dangerous and dirty business but the definable and fascinating problem of the flight of bodies through the air. It is not strange to find trajectories of roundshot and mortar shells

drawn by such an architect-engineer as Leonardo, trajectories that are in fact a good deal more realistic than his abstract ideas, but why did it become a mathematical commonplace? The answer is that, in its most general form, projectile motion had been since the days of Aristotle one of the unsolved problems of natural philosophy. Aristotle and common sense suggested that nothing moves without a mover, there can be no motion without cause. What is the cause of the free flight of a projectile, separated from the first cause of its motion? Again, according to Aristotle, unsupported heavy bodies fall down freely towards the centre of the universe, while only violent effort can force them to move away from the centre; the former motion being natural is accelerated, the second decelerated. What kind of a path does the projectile describe, then, to satisfy these conditions? Why, for example, when projected horizontally does a heavy body not fall straightaway toward the centre, but follow a sensibly horizontal line for a while, called by gunners the point-blank range?

One can discover attempted solutions to all these problems in the scattered notes of Leonardo da Vinci, who was an eager, self-taught, and naive philosopher as well as architect-engineer (and anatomist). None of his notes on motion represents an original idea; they are wholly unsystematic, and often contradictory. I use them as an example only. First we might observe that Leonardo confutes the still common belief that interest in projectiles came as a consequence of the invention of gunpowder. This belief is logically unsound and historically indefensible; it arises, I think, from a confusion about the history of firearms. It took only about half a century (that is, to about 1370) for cannon to replace mechanical siege engines—though the latter survive in books thereafter. It took well over two centuries for small arms to replace the crossbow in war.[9] Further, there was nothing very interesting about the behaviour of early bombards, whether built-up wrought iron cannon or the monsters used by the Turks against Constantinople; these were short range pieces. More interesting problems arose with the introduction of light bronze cannon firing cast-iron shot to longer ranges toward the end of the fifteenth century, and contemporaneously of mortar-bombs, with their indirect trajectory. However, these improved weapons did not create an intellectual difficulty that had been recognised for centuries, and which the mathematicians rather than the practical gunners attempted to solve. For experimental or imaginative purposes the crossbow was often the more convenient device; so Leonardo asks

whether if a bolt is shot from a crossbow four hundred *braccia* a crossbow made

[9] The crossbow was commonly employed for sport in the seventeenth century; as the Lancashire prodd it survived among poachers and others into the nineteenth; and it is still manufactured for sporting purposes at the present day.

> in the same proportions but four times the force and size will not send the bolt four times as far.

And again,

> I ask if a crossbow sends a bolt weighing two ounces a distance of four hundred *braccia,* how many *braccia* will it send one of four ounces? [10]

Here incidentally we may note Leonardo's propensity—the regular resort of medieval technology—to suppose that simple linear proportionality is applicable to every problem. One of the first practical successes of the new science of mechanics was to prove that this is not the case.

We also find common misapprehensions of fact stated by Leonardo, for example:

> In the centre of the direct path taken by heavy bodies which traverse the air with violent movement, there is greater power and greater striking force when an obstacle is met than in any other part of its line.[11]

Leaving aside the physical interpretation of projectile motion to concentrate on kinematics,[12] we note next that Leonardo embraced an analysis of the trajectory originating with the scholastic philosopher Albert of Saxony in the latter part of the fourteenth century, which analysis in turn derived from the Oxford and Parisian schools of the previous generation. The trajectory was divided into three portions, the first and last rectilinear, the middle curved. If a crossbow bolt is shot upward at an angle to the horizon, the violent motion it receives overcomes both gravity and the natural resistance to motion so that it flies straight. As the violent motion weakens, the trajectory becomes curved; when gravity and the resistance overcome the motion of projection, the projectile falls straight down to the ground. Many writers (including Tartaglia) depict such a trajectory. If the assumption is made, as it is by Daniel Santbech for example, that the first straight-line segment is always proportional to the force of projection, then the range becomes proportional directly to the charge and the cosine of the angle of elevation. The regular decrease of the cosine from angle zero to the right angle puts this rule at variance with experience, and accordingly

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[10] *Codex Atlanticus* 314v b; *The Notebooks of Leonardo da Vinci; Arranged, Rendered into English, and Introduced* by Edward MacCurdy (London, 1938), 1:531.

[11] Paris, Institut de France, MS A, 43v; MacCurdy, *Notebooks,* 1:540.

[12] As is now very well known, the impetus theory expounded with variations by all the most important writers on philosophy of motion in the sixteenth century (Tartaglia, Cardan, Benedetti, Buonamico, and the young Galileo) was of medieval origin, going back (probably through Islamic philosophers) to the Byzantines. See Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison and London, 1959) and Stillman Drake and I. E. Drabkin, *Mechanics in Sixteenth Century Italy* (Madison and London, 1969).

more practical writers on gunnery of the late sixteenth century proposed arbitrary mathematical schema relating increased range to increasing angles of elevation from zero upward.

The Italian mathematician Niccolò Tartaglia, who was born about 1500 and died at Venice in 1557, was the founder of ballistics since he devoted a whole book to it (*Nova Scientia,* 1537) and much of a second (*Quesiti, et inventioni diverse,* 1546). The former of these opens in the dedication to the Duke of Urbino with the following highly circumstantial piece of autobiography:

> When I dwelt at Verona in 1531 I had a very close and cordial friend, an expert bombardier at Castel Vecchio, . . . [who] asked me about the manner of aiming a given artillery piece for its furthest shot. Now I had had no actual practice in that art (for truly, Excellent Duke, I have never fired artillery . . . or musket); nevertheless, desiring to serve my friend, I promised to give him shortly a definite answer.[13]

This has always been taken *au pied de la lettre,* though I fail to see why Tartaglia's venerable bombardier should not rather be put with the Ancient Mariner, Shelley's traveller from distant lands, and countless Masters and Scholars of didactic dialogue. However, we can hardly suppose Tartaglia would have renounced experience had he possessed it.

The solution, resting on no very clear argument, is that 45° of elevation gives the extreme range. I have no doubt but that this was an intuitive result based on proportional symmetry; Tartaglia claims it was verified by trial. He also knew that complementary angles should give equal ranges, and claimed further that the extreme range is always ten times the point-blank. Hence he did not argue that the initial rectilinear segments are always equal at any angle. The curved segment he took to be an arc of circle to which the linear segments were tangential.

Tartaglia's theory is not much more than a dressed up version of Albert's or Leonardo's, and the mathematical garnish is really quite arbitrary. His most original contention was that no part of the trajectory—not even the point blank—is truly rectilinear; yet in geometry he always treated it in the way I have described. His conceptions are Aristotelian ones, modified by the impetus theory. As Koyré has remarked, it was exceedingly difficult in these matters to step outside the tradition, and in so far as Tartaglia departed from it—especially in abandoning the idea of strictly rectilinear segments—this did not help him to solve the geometrical problem.

The recondite and sometimes absurd philosophical arguments that

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[13] The translation is by Stillman Drake. Drake and Drabkin, *Mechanics in Sixteenth Century Italy,* pp. 63–64.

constitute the greater part of Tartaglia's writings on ballistics were of no value to the compilers of practical manuals on gunnery, and though these writings of Tartaglia were plagiarised in other vernacular texts, it was necessary if range-tables were to be given—for Tartaglia, having promised them, did not give them—to derive them in some arbitrary fashion. Thus Diego Uffano, "captain of the castle of Antwerp," proposed a simple arithmetic series increasing the range steadily from zero to 45 degrees; if the point-blank range was 200 paces, he said, then at each degree of elevation the range would be 244, 287, 329, etc., to a maximum of 1,190 paces. The only real interest in these arbitrary tables is that they prove how great an authority mathematics possessed. There is no reason to believe that they were ever used, or were usable. But they made excellent propaganda. In some of the treatises on gunnery, and many other books on applied mathematics, great emphasis was laid on the importance of the arts of measuring distance, heights, depths, of preparing plans and maps, and of familiarity with simple arithmetic and geometric rules, to the scholarly or gentlemanly soldier.[14] The writers of these books have in mind a figure who is not by any means the engineer-architect of the earlier Renaissance, who was not a leader of men in battle or tactician, but a noble, scholarly soldier who shall be master of the established mathematical arts, and also of the mathematical art of gunnery, as well as of all the practical aspects of warfare. So Thomas Digges writes (*Pantometria,* 1571): "for science in great ordinance especially to shoote exactly at Randons (a qualitie not unmeete for a Gentleman) without rules Geometrical, and perfect skill in these mensurations, he shall never know anything."[15] Such a gentleman-artillerist was perhaps too ideal a figure, but other writers insist that the gunner have skill in surveying and so forth to raise him above the ordinary level of under-officers. Even so, some study of accounts of battles on sea and land suggest that the average good gunner was a man who knew how to load and fire his piece efficiently and safely, and while aiming by line of sight make such allowance as experience and trial suggested for long range and other factors. We have to remember that seventeenth century cannon were very idiosyncratic and irregular in their shooting, each gun being made from a unique mould, that the charge and quality of the propellant was highly variable, and the projectile occupied only 90 per cent of the area of the bore. Consistent

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[14] See Peter Whitehorne, *Certain Waies for the Orderyng of Souldiers in Battelray and Setting of Battailes* (London, 1562); Cyprian Lucar, *Three Bookes of Colloquies* (London, 1588); and Walter Ryff, *Der Furnembsten . . . Architectur . . .* (Nurnberg, 1547).

[15] Sig. A iii. "At randons" means, elevated above the point-blank (hence, tellingly enough, the more usual *random*).

practice was impossible, and rules likely to be less effective than the good gunner's experience and correction of his aim.

Some historians (Edgar Zilsel and recently Christopher Hill) have made much of the existence of a whole group of superior artisans at this period, from the architect-engineers through painters and musicians to gunners, surveyors, navigators, cartographers, and instrument makers, apothecaries, opticians, clock makers, and so on. Obviously, levels of craft skill did exist; some crafts employed simple mathematics, others chemical knowledge. Clearly too these superior craftsmen contributed to the refinement of technology. But one should be cautious of taking "mathematics" in too grandiose a sense; one should remember that gunners and sailors were simple men, and that, for sure, they were consumers, not creators, of mathematics.

However, we can now see how well the world was prepared for at least one feature of the kinematical discoveries of Galileo; the writers on practical mathematics and artillery had long been confident that their art must follow some rational mathematical scheme, and they were prepared to believe that Galileo had discovered it. The writers did not test his theory by experiment nor enquire about its application in the field; it was enough that the new theory looked right, even if sometimes the explanation of its curved trajectory harked back to Tartaglia, rather than Galileo himself.

As everyone knows, Galileo rediscovered and applied to actual bodies falling at the surface of the earth the square-law of acceleration; he understood perfectly the vectorial combination of motions, and this gave him the parabola as the path of a projectile—neglecting the curvature of the earth itself. Accordingly, at the end of his *Discourses on Two New Sciences* (1638) he was able to produce that great desideratum, a theoretical range table, and a number of accurate propositions about projectile motion. This work on ballistics was developed further by Galileo's pupil, Evangelista Torricelli, who generalised and completed the theory, after which it passed into general circulation.

What was Galileo's interest in solving the problem of projectile motion, which as we know occupied him for over thirty years? To my mind the utilitarian aspects of the *Two New Sciences* have been grossly exaggerated. Galileo was above all a mathematical philosopher; most of his life work was devoted to the general theory of mechanics, not to say astronomy and cosmology. But he liked to display his abilities in the most direct and conspicuous fashion. There can be no doubt that the *Two New Sciences* was written to demonstrate the falsity of the simple rules of proportion followed in the old craft tradition, and the superiority of the new, philosophical laws devised by Galileo himself. He

was not, so to speak, on the same side as the artisans; he was proving that the philosopher understood things *better* than they did. For a century and more, gunners had fumbled at the mystery of ballistics; Galileo's new treatment of kinematics unlocked it at once. Galileo was quite explicit about this. In 1632, after Bonaventura Cavalieri had first put the parabolic theory in print, he complained to a friend about the loss of "the renown, which I so keenly desired and had promised myself from my long labours" in mechanics, saying that to master the trajectory of a projectile had been their chief objective. It was, after all, the most celebrated of all problems in mechanics, quite apart from any question of the usefulness of its solution.

Did Galileo believe his own solution to be useful? If we suppose Galileo to have been drawn all along, as Tartaglia said he himself was, to a practical problem of artillery, and if Galileo really thought that he had solved this practical problem, then the answer clearly is, Yes. Certainly Galileo talked about his ballistic theory in a very practical way. But he was also quite aware that when movements are very swift they are greatly impeded by the resistance of the air: this resistance was especially strong in the case of musket and cannon balls. "The enormous impetus of these violent shots," he wrote, "may cause some deformation of the trajectory, making the beginning of the parabola flatter and less curved than the end;" but so far as this book is concerned, he went on, "this is a matter of small consequence in practical operations, the main one of which is the preparation of a table of ranges for shots of high elevation . . . and since shots of this kind are fired from mortars using small charges . . . they follow their prescribed paths very exactly." [16] Hence Galileo correctly enough supposed that the parabolic theory could have a limited application. However, he was by no means always scrupulous in making this clear—his tables do include the small angles—while Torricelli was even more realistic in his language, thereby creating the impression that the parabolic theory had completely solved the problem of exterior ballistics, at least in principle. When challenged, Torricelli attributed discrepancies in practice to the imperfections of guns and gunners, being seemingly reluctant, unlike Galileo, to admit that a large physical factor had been omitted from the parabolic theory.

Later writers on the theory of gunnery until well on in the eighteenth century were content to rely on this beautifully idealist conception, which became general from about 1670 onwards. Such influential "practical" treatises as Robert Anderson's *Genuine Use and Effects of the Gunne* and François Blondel's *Art de jeter les bombes*

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[16] Galileo, *Dialogues Concerning Two New Sciences,* trans. by H. Crew and A. de Salvio (Evanston and Chicago, 1946) , p. 246.

appeared in 1674 and 1683 respectively. Blondel's book is very thorough both in its critique of older ideas about the flight of projectiles and in its exposition of the parabolic theory; here he called in some of the mathematicians of the Académie Royale des Sciences to solve its most abstruse proposition. Blondel claims that the theory is most exactly applicable to mortar fire but does not exclude its use for cannon, nor admit plainly that air resistance is a disturbing factor. In 1731 Belidor's *Bombardier François* employs the parabolic theory, limiting his tables strictly to mortars. Benjamin Robins in 1742 was the first to show decisively that the parabolic theory was inadequate for all but very slow projectiles.

The mathematical study of the motion of bodies in resisting fluids was by this time two generations old, since it had begun with investigations by James Gregory, Wallis, Huygens, Newton, and others beginning about 1670. To connect these investigations with artillery practice at that time seems to me wholly unrealistic, though of course I do not deny that the mathematicians were conscious of the fact that artillery projectiles like ships did exemplify resisted motion. As I remarked before, one must remember that military orders commonly forbade gunners to fire at other than point-blank range, especially at sea; commanders were sceptical, to say the least, of any attempt at long range practice, except with mortars.

To sum up, it seems to me that the historian of any branch of technology must be careful not to read the present back into the past, nor to credit the writings of armchair specialists and propagandists without some other check that such authors describe things as they are, and not as they might be. Just as there have always been soldiers, artists, and industrialists of the no-nonsense brigade who have dismissed all attempts at rational theorisation (whether mathematical in form or otherwise) as absurd and needless, so there have always been experts trying to convince the world that they alone hold some particular theoretical key to reality. Time has proved the philosophy of the latter group correct. Everyone today knows what abstruse computations enter into the calculation of trajectories; fundamentally the methods used today go back historically to the theoretical mechanics of Newton. But there was not either in principle or in historical fact any role for theoretical mechanics to play in the warfare of the seventeenth century, and any mathematics used was of a most trivial kind. As in attempts to put physiology and medicine on a chemical basis, or to construct a machine enabling man to fly, the imagination of the seventeenth century ran forward to what was realised only in the twentieth. But we should not overrate the importance of such imaginative foresight, or conclude that experimental research and technological invention have always been exclusively devoted to turning such visions into reality. It is always

dangerous to disregard the force of tradition, and the strong conservative elements in even the most original minds. In both science and technology many of the most persistent and ultimately the most fruitful of problems have been traditional ones, tackled in different manifestations by successive generations.

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Commentary

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I agree with the conclusions of Professor Hall's delightful paper and I want, if I can, to confirm them by coming to them via a different route. This route has three lanes: the organization of armies; the mentality of soldiers; and a conceptual lane.

The modern armed forces expect science to help them. This notion begins in army schools and carries through the whole education programme sponsored by the military: military academies and universities, staff college, special courses for serving officers. On behalf of the military the government employs or retains scientists and underwrites research projects. There is liaison with the research going forward in industry. The military have testing grounds where the technological applications of scientific theory can be evaluated, they can test new methods of, say, communication, through large scale manoeuvres. The notion that there is an interconnection, science-technology-warfare, is kept constantly in the career soldier's mind through military periodicals, lectures, and refresher courses, and the short-term soldier has at least enough training to take the role of the boffin for granted.

Matters were, of course, very different in the sixteenth and early seventeenth centuries. And the first argument I want to suggest is that armies were unable to look to science and its technological fruits because of the way in which they were organized.

In most armies the greater number of men had been picked up out of civilian society and at the end of a campaign were dropped back into it. They were given a minimal training, and sometimes none, before going into action. The captains who recruited and paid them had almost

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* As this comment was prepared when I was away from my books and notes, and as it depends more on argument than illustration, I have refrained from adding a full battery of footnotes and have merely checked the text and identified quotations. For a general background to the issues touched on here see my chapters on warfare in volume I (1957), II (1958), and III (1968) of the *New Cambridge Modern History*.

** Since delivering this commentary, Professor Hale has been appointed Prefessor of Italian at University College, London.

as much incentive to let them be killed as to instruct them how to kill others; on campaign it was easy for a captain to conceal losses and go on drawing pay on their behalf. Pay was in any case very poor, and it was habitually in arrears. Death through disease was far more likely than in battle and proportionally far more likely for the soldier off the battlefield than for the civilian. Commissary services were usually inefficient and the soldier was frequently reduced to scavenging for himself. Uncertain pay, uncertain rations, disease probable, hardship inevitable on long marches over foul roads: the conditions of service were such that captains could exact only a minimum of cooperation from the soldier, certainly not enough to turn him into an educated one. Most were, in any case, illiterate.

Next, numerically, were the mercenary companies, better soldiers, used to fighting together, with pride in their corps and loyalty to their commanders. But even among mercenaries the military life was looked on by—I suspect—the majority primarily as a means of weathering hard times or of accelerating social mobility and getting back as soon as possible into civilian life with money saved and status enhanced.

Fewer still were the full-time, free-lance soldiers who kept on the move about Europe from war to war. Often of good family, some of them really well educated, highly professional, they tended to be prickly over matters of rank and precedence, cherishers of chivalrous observances, and traditionalist in their military thinking.

Fewest of all, the colonels of regiments and generals of armies were still chosen more frequently for their social status then for their military experience. Controlling armies that were usually multi-lingual and multi-national and always plagued by administrative and financial problems, their main concern was, in any case, to keep discipline and get an army onto the field in some sort of order. Neither their background nor the nature of their function encouraged them to be innovators in military affairs.

Much discussed in theory, and experimented with here and there in practice, as in Sweden, standing armies accounted for a very small proportion of the men actually under arms at any moment. Permanent bodies of troops, allowed for by on-going tax allocations, can be divided into two categories: garrisons in fortresses or fortress towns, and royal bodyguards. The conditions of garrison life, where troops were commonly on half pay and were seldom turned out as a body except for the calling of the muster roll, produced no contribution to military thought that I know of. Royal guards were expected to be loyal, smart and, in action, ferociously brave; but nothing encouraged them to use the

permanence of their employment or their contact with the ruler and his ministers to rethink any aspect of military practice.

The insulation of armies from the worlds of technology and science was, in fact, even more complete in peacetime than in war. At least during campaigns that lasted many years, such as those in the Netherlands in the late sixteenth and early seventeenth centuries, there was occasion for the exchange of ideas between the engineers and artillerists of different countries, for the trial and error tactical adjustments that led, for instance, to the development of the infantry square and the fire-and-wheel-about evolution of mounted pistoleers. But when peace came an army as good as vanished; the short-termers went home, the mercenaries marched off to the next job, the free lance rode his horse in search of another trouble-spot—he was always welcome on the Habsburg-Turkish frontier—and the generals returned to their estates or their civilian offices under the crown.

Now peace was more commonly the result of financial exhaustion than of battle or siege. Overwhelmingly the tax structure of western Europe had been shaped by the needs of war. Taxes hit the poor more than the rich, and wars commonly ended with food riots at home and the possibility of social revolution. There was every incentive, then, to cut military expenditure to zero as soon as hostilities ceased. The classical tag, "In peace prepare for war," was much bandied about but could not be acted upon. Government-controlled gun foundries were starved of money for testing, let alone experiment; the militia companies, legally kept in being in peace and expected to turn out for regular training, were starved of powder and shot and frequently were allowed only enough powder to flash in the pan. When war began again the old equipment was hauled out of storage and contracts given to armourers and gun-founders who could produce traditionally proven weapons as quickly as possible.

I have tried to give a picture—necessarily impressionistic and incomplete—of armies doomed to inefficiency and thus inimicable to experiment, short-budgeted, lacking in continuity of personnel, linked to industry only intermittently (and then through non-military contractors), organizationally closed against any interrelationship between the discoveries of science and the needs of war.

There was one exception to this closure. Inventors frequently wrote to governments claiming to be able to revolutionise the art of war. In Venice, for instance, one of the functions of the office of the *Provveditori* of fortresses was to examine such claims and arrange for trials to be made or models constructed, if they thought it worth while. Of the inventions submitted between 1578 and 1630—mainly for earth-shifting

equipment, wall construction, new types of propellant, and diving apparatus—while many were tested, only one was adopted, the reason usually being that even were the invention workable, the improvement on traditional methods was too slight to justify the expense of standardizing new equipment. And none of the inventions, it seems to me, involved the exploitation of a new scientific discovery. A channel of communication did exist, then, between an Italian military organization and the world of Italian science in the age of Tartaglia, Galileo, and Torricelli, though the office was reluctant to spend money and was staffed by civilians; but there is no evidence that scientists, as opposed to cranks and the adaptors of familiar mechanical and chemical practices, wished or were invited to use it.

The lack of a peacetime establishment so financed and so motivated as to strive to improve methods of warfare is surely fundamental to the problem we are examining. Yet Queen Elizabeth's great minister Lord Burghley—one of whose jobs was the examination of projects submitted by inventors—warned his son Robert against thinking of military careers for his children because, as he said, war "is a science no longer in request than in use. For soldiers in peace are like chimneys in summer."

He also gave a further reason: "Never, by my consent, shalt thou train them up in wars. For he that sets up to live by that profession can hardly be an honest man or a good Christian." [1] And this brings me to the second point I want to suggest. An army must not only be organized in a particular way to enable it to take advantage of advances in science and technology, but it must contain men ready to look out to such advances. And this readiness depends, in part at least, on a feeling of confidence with regard to their status in the eyes of the civilian world where the scientists and the technologist-craftsmen live.

Throughout the period I am talking about, the prestige of the military career was slipping. Formerly it had acquired lustre from its association with the Second Estate, the class of nobles and gentlemen whose chief function was to fight to protect the other members of society. Increasingly, however, this class had opted out of their military role and turned to estate management, the law, the church, or government service. Already by the end of the fifteenth century Caxton was complaining that gentlemen didn't bother to train themselves for war; [2] in 1622 Henry Peacham's *Compleate Gentleman* assumed that his hero would only want to take up arms in an emergency. And while armies were losing their aristocratic and chivalrous associations at the top, the

[1] Quoted by Joel Hurstfield, *The Queen's Wards* (London: 1958), p. 257.

[2] Epilogue to *Order of Chyvalry* (? 1484).

image of the ordinary soldier was steadily coarsening. The stalemates of sieges produced longer campaigns, and these threw into higher relief the effects of poor supplies, arrears of pay, and the spread of disease. Soldiers—this was in Burghley's mind—became identified with theft, looting, cruelty to civilians, and with downright mutiny, as when the crack Spanish garrison of Antwerp ran amuck in 1576.

To refurbish this image, literate military men produced a sizeable flood of books stressing the high nature of the soldier's calling and the equally high nature of the personal qualities required for its pursuit. These books give us a considerable insight into the professional military mentality of the period.[3]

Overwhelmingly, this mentality was inward-turning and traditionalist. The chief concern was not with weaponry but with discipline and morale. What was needed, it was felt, was not a change of equipment but a change of heart; and in pursuit of examples of great-hearted armies the authors were led back in time to the victories of, say, Agincourt, but mainly to those of ancient Rome. This preoccupation with the ancient world was not due only to the sensational achievements of the well disciplined armies of Alexander, Hannibal, and Caesar, but to the desire to tap the prestige humanism had given to any reference to antiquity; for a second aim of the military writers was to stress the notion that the good soldier must be an educated man.

In doing this, they were following the example of another occupation group that had already managed to improve its social image by pointing to the wide educational attainments expected of its members. I mean artists—painters, sculptors, and architects—who from the middle of the fifteenth century had begun to edge away from their status as mere craftsmen by drawing attention to their need to be acquainted with such liberal and socially respected subjects as history (for choice of subject and accuracy of detail) and above all (for perspective and proportion) mathematics. Together with the history, literature, and philosophy of the ancient world, mathematics, and especially the part of it concerned with geometry, was one of the most fashionable subjects of study in Renaissance Europe. It was part of the aristocratic syllabus of study and it is not surprising that the military writers emphasised its applicability to their own profession.

It is from this emphasis that we get the impression that war was becoming more "scientific" in the sense of becoming more rational and orderly. The books give beautifully precise diagrams for wedge-shaped,

[3] See M. J. D. Cockle, *A Bibliography of English Military Books up to 1642 and of Contemporary Foreign Works* (London: 1900).

cresent-shaped, square, and saw-toothed-shaped infantry formations; tables for the number of troops required to defend a particular area of ground; tables for gun ranges; and the formulas necessary for the engineer-geometer to plan fortifications.

Except in the case of fortification, this was nearly all an effort of image-building rather than a reflection of what actually happened. A company was never drawn up in a saw-toothed formation on the battlefield; a gunner, as Professor Hall pointed out, relied on knowing the idiosyncracies of his piece rather than on his quadrant, if he had one. And though at least the standard of mathematics required of the surveyor was also needed by the engineer who designed fortifications, it is interesting that throughout the literature on fortification there sounded an apologetic note: apology for an art that took for granted the existence of an unchivalrous agent, gunpowder, apology because the construction of fortification involved a thoroughly Third Estate activity, soil shifting.

And if we try to get at the military mentality by listening to the printed dialogues between soldiers—a form in which many of the military books were cast—or, more usefully, at the dialogues in memoirs and memoranda not intended for publication, we catch a mood that was similarly inimicable to the search for ideas outside the ranks of the military themselves. There is much technical controversy over, say, the proportion of pike to shot in an infantry square, over the respective merits of round as against pointed bastions. There is much talk of "the rules" of war, much deference to the opinions of "experts," but there is no hint of a desire to look outside the profession itself for technological improvements, let alone any suggestion that there was something called "science" to invoke the aid of. And I think it is not altogether fanciful to add to this disinclination of the military to look outward a feeling among civilians that the military had no business to be dabbling with technological experiments. Writing for the theatergoers of Puritan London, Ben Jonson poured ridicule on the gadgets produced by Spinola's engineers in the Netherlands by inventing the rumour they were issuing the army with cork boots so that they could walk to England across the waves.[4]

More serious than this conjecture is the third suggestion I want, in all brevity, to make. Soldiers can only look out towards science when they have a concept of what science is, and what it does. I doubt whether such a concept was available, for military men at least, until the scientific academies of the mid-seventeenth century started publishing their findings. Nor, I suspect, did they have any conceptualized

[4] *The Staple of News,* III, i.

attitude to technology. It was known, of course, that refinements were being introduced within certain industries, glass making, for instance, or metallurgy, or in the extractive industries; but these isolated pieces of technology did not cluster together in men's imaginations to produce the image of an advancing technology that had something to offer to whoever approached it. This was partly because the very idea of "progress," man's ability to dominate and improve his environment and put natural laws to work for him, had not yet achieved enough momentum to wrap up the various separate technical advances into a point of view. Until it was possible to have a concept of progress and within it, as it were, concepts of science and of technology and of their interrelationship, it was difficult for the soldier to see these activities as potentially at his service. Meanwhile, he would accept the invention of a craftsman, like the wheel-lock, or lend a gun to a scientist interested in ballistics, without a hint of the programmatic approach we are used to bringing to bear on the triad of this Symposium, science, technology, and warfare.

Let me in conclusion try to pin down this last point with an example. In the sixteenth century, Tuscany was ruled by two princes, both of whom were amateur scientists, Cosimo I and his son Francesco. Both had laboratories of their own and were particularly interested in chemistry. Their example was followed in the next century by Ferdinand II and his brother Leopoldo, who were responsible for founding the first organized scientific society, that of the Cimentisti, or the experimenters. Cosimo and Francesco were both, moreover, interested in technology, particularly with regard to shipbuilding, irrigation, the composition of ceramic compounds, glass-making, and the treatment of crystals by heat. Both men, again, were the titular heads of armies and took a close interest in fortification; court artists portrayed both men giving orders to military engineers. Francesco's brother Giovanni was not only an experienced soldier but was responsible, together with the engineer-architect Buontalenti, for designing the Belvedere fortress that overlooks Florence itself.

Here then we have what might have been a model environment for the interaction of war, technology, and science: a small and not too badly paid standing army; an absolutist state whose rulers had scientific and technological interests, a magnificent collection of scientific instruments, and foundries and workshops under their direct supervision. Yet no advance in weaponry, explosives, or even in methods of fortification emerged. The three elements of our discussion coexisted in Tuscany without interpenetrating.

Given then the way in which the armed forces were organized, the

mentality of the soldier, and the absence of certain key concepts, it is not surprising that the flow of ideas from science via technology to war, or the impact of the needs of war on technology and science, were at best sporadic and never amounted to a positive programme.

Commentary

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The splendid papers by Professors Hall and Hale are striking evidence of the excellence of British scholarship. There surely can be no serious question about the soundness of their thesis that science, as we understand the term, had little or nothing to do with warfare in the period that they discuss.

We have been told that Galileo developed firing tables for artillery, that the master engineers who built fortifications relied upon geometry, that Colbert presented the *Académie des Sciences* with problems of shipbuilding, gun forging, etc., but obviously much of this information must be taken with a grain of salt. The guns of Galileo's youth were so idiosyncratic that each had its own name, its own characteristics, and its own gunners who, often enough, had also directed its casting. In his old age there had been a considerable multiplication of the number of guns in Europe, but no standardization that would permit the use of firing tables. Indeed, it was not until the mid-eighteenth century that relatively standardized guns began to appear, and even as late as the 1880s the gunner had to know the individual characteristics of his weapon and regulate the charge of powder necessary for the best results. The same order of things was true for fortifications: at Rocroi or New Breisach, Vauban had flat land on which to project his geometric patterns for fortification, but a glance at Namur, Lille, Philippsburg, or a dozen or more other seventeenth century strong points will show how individualistic the problems were. What was needed was a minimum of geometry and a maximum of sound engineering common sense. The same was true for the naval evolution of the seventeenth century that produced the first flexible sailing vessels with effective batteries. These ships were the product of the master-craftsman builder, not of the scientist.

However, as Professor Hall notes, technological developments did have important consequences for the art of war. Of these none is more important than the emergence of effective artillery as a decisive factor at sea and an important one on the land. It is a fascinating story that cannot be fully explored here. The forged guns of the fourteenth

and fifteenth centuries were followed by cast bronze cannon and finally by cast iron ones. Western European craftsmen and gunners reversed the fifteenth century trend toward ponderous, giant weapons in favor of smaller, more flexible guns. The first of the latter were cast by men who made bells, aided in the casting by the gunners themselves: both were skilled workmen who operated on a rule of thumb basis. We should note, in passing, that these men were as valuable to princes as scientists are to modern governments, and that their services were eagerly sought after and handsomely rewarded. The gun foundries first appeared in the Walloon Netherlands, southern Germany, eastern France, and northern Italy. Men from these areas were lured away to establish works in England, Spain, Portugal, Sweden, and even far away Russia.

The important seventeenth century story was the development of the cast iron gun. Even as late as the American Civil War gunners recognized that the best guns were made of *"fonte"* or bronze: they were lighter, less apt to explode, and less susceptible to corrosion. However, iron is less expensive than copper and tin, and as the governments of princes came more and more under the direction of men of the pen rather than of soldiers, this fact grew in importance. Early attempts to cast guns of iron were largely failures; the methods used for casting anchors or cooking pots were not adequate for cannons, but in time, the ironmasters learned how to eliminate some of the impurities in their metal and to cool the guns slowly, thus casting with fewer faults. Trial, error, and accidents had much to do with the process. For example, the English gun makers of the early seventeenth century became the most important in Europe, probably because their iron ore was mixed with phosphorus rather than with sulphur. It may also be that the large domestic "private market" for cannon in England, where predatory patterns easily led to piracy, may also have accounted for the fortunes of the English cannon makers. By the mid-seventeenth century, when the English crisis in forest products slowed up the English ironmasters, Swedish cannon makers assumed first place in the European markets. However, a glance at the Amsterdam market, where the munitions trade of the seventeenth century was largely centered, will show that by 1670 iron guns were being cast in many parts of Europe. We may safely assume that these weapons were the product of master craftsmen. The ironmasters, like the rest of men, knew no chemistry, and the evolution of the guns suggests that their shape was the result of the advice, perhaps of the direction, of master gunners who even by 1650 were not yet completely accepted as soldiers, but whose role in sieges and in field warfare was becoming ever more and more important.

In the latter seventeenth and the eighteenth centuries the manu-

facture and testing of these guns was often carried out in arsenals under the supervision of the ministry of war or marine, so that engineers and bureaucrats took a place in the development of the gun. The process, however, was slow. By the mid-eighteenth century French cannons were standardized so that the balls more exactly fitted the bore and could be interchanged from one gun to another. About the same time in Germany men learned that by reducing the charge of powder it was also possible to reduce the weight of the cannon. Thus a four pounder was reduced from about 600 kgs to about 300 kgs; a 12 pounder from 1,600 kgs to 900 kgs. This gave much more flexibility to the artillery, especially since the reform was accompanied by better caissons, new harness for the horses, new methods of sacking the powder, and more effective hardware for servicing the guns.

Another interesting aspect of the evolution of the cast iron gun was the emergence of heavy mortars that could be mounted on shipboard or carried along with the siege train. There was a significant improvement in these weapons about 1680, when they became the weapon for the bombardment of cities with explosive shells both from the sea and from the land. One has only to see what happened to Genoa, Algiers, and the Flemish towns in the early 1680s, or the poundings that the Anglo-Dutch navies gave the French Channel ports in the 1690s and later in the War of the Spanish Succession. The bombardment of these French ports was responsible for the first proposal, to my knowledge, for the recognition of the immunity of "open cities" from bombardment. In the eighteenth century the Prussians had mortars light enough to be used in the field. Their effectiveness was definitely proved at Rossbach when the mortar fire from behind a hill worked heavy damage on the French army.

The cannon delivered solid shot or a sort of grape and cannister; the former was effective against fortifications and troops in column; the latter was often a devastating stroke against a line. The mortars could launch an explosive shell, but without penetrating power. By the opening years of the eighteenth century the howitzer was also known, but its explosive shell was not perfected until the end of the century when better methods of setting fuses came into existence.

I cannot emphasize too much the importance of these guns on the development of the art of war. By reducing the cost of each by a third to a fourth, and by using plentiful iron rather than scarce copper and tin, the cast iron gun enormously increased fire power. Without them the great fleets of the latter seventeenth and eighteenth centuries would have been impossible. Even though the bronze gun still had greater prestige, three-fourths of the guns on the fleets of 1690–1789 were cast

iron. In 1600 artillery was used largely at sea and for sieges; the guns were too heavy and awkward for successful field operations. During the Thirty Years' War, Gustavus Adolphus introduced the first useful cast iron fieldpieces. They were small in caliber, but they could be fired more rapidly than an infantryman could fire his piece. Of course they did not immediately revolutionize the battlefield; indeed, the pike was still "queen" and did not disappear until the end of the century. Nonetheless, these guns were evidence of a trend: by 1688 the French army had a goal—not really reached to be sure—of one gun for every thousand men on the field. By the end of the century it had become four to a thousand. In 1688 the king's army numbered about 100,000 men; by 1705 it was over 400,000 men. They could not have been armed entirely with bronze guns, but with cast iron it was possible.

Need I point out that these guns made inroads on the traditional methods of fighting? One has only to see the French batteries at Malplaquet mowing down the Dutch regiments with ferocious fire, or the Prussian and Austrian batteries in the mid-eighteenth century, to realize their importance. Of equal moment was the significance of the new firepower at sea. It gave European sailors absolute advantage over the ships and harbors of the Near East and the Orient and played an important part in the wars between Europeans during the latter years of Louis XIV and the eighteenth century.

Perhaps of equal importance was the development of more effective weapons in the hands of the seventeenth century infantrymen. By 1500–1550 firearms had completely displaced the cross- and longbows, but until the Thirty Years' War they were not effective enough to make the cavalryman's caracole a dangerous maneuver, and musketmen needed the protection of pikemen against a cavalry charge. Thus in 1650 the infantry regiment was a mixed company of pike and musketmen. While they needed each other they did not make a very flexible or effective weapon. No one was more harmless than a pikeman as long as the horses kept their distance, and the pikes were no longer useful in a battle between infantrymen. There were two problems to be solved: the musket had to be made more effective, and protection against cavalry, other than the pike, had to be found. By the middle of the seventeenth century, gunsmiths had perfected the weapon needed for better firepower, but soldiers were too cautious to use it. Sportsmen knew the flintlock, or fusil, and many individual soldiers tried to arm themselves with this weapon; but war ministers like Le Tellier and Louvois, and soldiers like Turenne, preferred the matchlock both because its effective range was longer than the fusil, or flintlock, and they feared that the latter would not fire if the weather turned wet. Thus the

French soldiers were forbidden to have the fusil. It should be noted that this order was not universally obeyed, and finally Louvois did admit that a small part of the troops could use the weapon. It obviously was more flexible than the old musket; it was almost half as heavy and it could be fired twice as fast. The imperial army adopted the flintlock during the Hungarian war of the 1680s, but the French did not accept it until after the death of Louvois. Luxembourg sent his son to Versailles after the battle of Steinkirk to explain to the king that French infantrymen threw away their pikes and their matchlocks whenever they could take a flintlock from the enemy. Louis was convinced and ordered the changeover to the fusil for his army. It was the standard weapon for the eighteenth century. Indeed, as we shall see, it was not fully exploited until the end of that century.

The battle of Steinkirk also saw the disappearance of the pikemen. In the early sixteenth century the thrust of the pike was the queen of the battlefield; by the mid-seventeenth century is was simply protection against horsemen. It took no great imagination to understand that a sword attached to the musket could do almost as well as a pike, but the first efforts to solve the problem were not promising. The "plug" bayonet deprived the infantryman of his best weapon, the projectile; indeed, one of the last battles to be won by swordsmen over musketmen resulted from the embarrassment caused by the plug bayonet. Again the problem seems to have first been solved in the Hungarian War. Karl of Lorraine tried several kinds of barriers for the protection of musketmen against cavalry before the army finally adopted the strap bayonet. This weapon encouraged the development of the three or four line infantry volley that became the standard practice for the eighteenth century.

The infantryman also became more effective as the result of the development of better cartridges, an iron ramrod, and new methods of loading his weapon. He could not always stand up against a massive cavalry charge like the one at Ramillies, but increasingly the foot soldier provided a barrier behind which his own cavalry could reorganize when it was hard pressed. A determined volley by the footsoldiers could do terrible damage to a cavalry squadron, and the bayonets were effective against all but the most determined cavalry assault. Malplaquet, Oudenarde, and Denain provide striking evidence of the importance of the new infantry regiments.

Several French historians insist that the great military problem of the eighteenth century was concerned with the evolution of tactics suitable for the fusil. The infantrymen of the first years of the War of the Spanish Succession formed a loose line because they still remembered

the old matchlock musket that had a tendency to explode. Thus the volley was weakened. Gradually the line tightened to a point where the volley did present a wall of fire. This was the fusillade that taught Frederick II that firepower was the queen of the battlefield. Soldiers marching on the double against a firing line had little chance of success unless they also presented a wall of fire. This was the origin of Frederick's "moving wall of fire"; his soldiers were trained to march and shoot. Other armies found it difficult to imitate this maneuver for it required much discipline and rigorous practice. The French officers were sure that it did not correspond to the genius of their nation, but their response by an attack with a line fifty ranks deep was a disaster, for only the first lines had the shock effect, while the whole body was exposed to enfilading fire by both cannon and fusillade. For similar reasons the French armies of 1914 suffered a comparable debacle on the Alsacian front.

The fusillade, however, was not the most effective use of the fusil. The soldiers were trained to fire on command rather than at a target. This was one of the reasons that soldiers ignored the rifled weapon. They depended upon the "wall of fire" rather than upon the individual shot—indeed, Frederick's troops were trained to aim at the ground some fifteen feet ahead of the enemy on the assumption that the kick of the fusil would result in a hit. In the latter years of the century soldiers learned that the fusillade fired on command was effective for the first volley, but thereafter more telling execution came from allowing the troops to fire at will rather than at command. It was better to shoot to kill rather than merely in unison. Several important improvements in the weapon that made this tactic viable were the iron ramrod, better cartridges, and fusils that were better standardized. These seem to have been the work of gunsmiths and sportsmen rather than scientists.

There were other technological developments that affected the art of war. A small example might be found in the portable bake oven that came into use in the French army toward the middle of the Dutch War. Louis XIV ruefully tells us that the campaign of 1672 might have turned out differently had his troops been supplied with bread baked in the field, rather than depending upon the villages and towns; but this is a questionable point, for even toward the end of the eighteenth century field ovens were unable to supply all the bread and biscuit needed by an army. There were also improvements in pontoon boats as well as boats for river transportation, but the number needed never seemed to be available. The tools for mining and trenching, developed by siege engineers, also became more effective. One of my students has recently

published a translation of Vauban's "Do it yourself" book on making and taking fortifications, which indicates some of these developments.[1]

One thing that no seventeenth or eighteenth century army solved was the problem of carting enough forage; in every war both sides paid close attention to the ribs of the horses, ridden by "trumpets" bringing messages or proclamations from the enemy, as an indication of the conditions in the enemy's camp. By the middle of a campaign there seldom was enough food for the animals, and failure of forage had the same effect on a seventeenth century army that a gasoline failure would have in our days.

I wish that my field of competence were greater than it is for a discussion of naval problems. The latter fifteenth and sixteenth centuries witnessed an important naval development. The sailors of the Atlantic coastal states learned to substitute the sail for the oar, and the cannon for the ram, and thereby opened a new chapter in naval history. The galley had been the effective warship since the beginning of naval warfare; it was now superseded by a weapon that relied less on manpower for both mobility and assault. These new warships were constructed much the same as the commercial vessels of the day. Indeed sixteenth century warships and commercial ships were almost interchangeable. In the seventeenth century, however, they became more specialized; but there seems to be no good evidence that they were the result of anything but the master-mechanic shipbuilder's conception of proper design. Indeed the ships of each nation reflected the problems that confronted the shipbuilder who made ships for merchants as well as for princes. In the eighteenth century this practice continued in most of the shipyards of Europe, but in France toward the mid-century naval engineering as a more exact art, perhaps even as a science, brought substantial changes in ship construction. The result was that in the second half of the century French warships were the best on the seas, and their enemies were always anxious to capture them for their own use. It should be noted that these mid-eighteenth century warships were probably as good as any produced before the advent of steam.

There were other significant developments at sea. The use of copper sheeting on the hulls of ships gave them more speed, but even more important seems to have been better methods of setting the sails. By the eighteenth century a ship could sail nine to twelve degrees into the wind; this made it much more effective as a weapon. There also were improvements in the art of navigation that seem to have been in part the

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[1] Sébastien Le Prestre de Vauban, *A Manual of Siegecraft and Fortification,* ed. and trans. by George A. Rothrock (Ann Arbor, 1968).

result of advances in astronomy and the technology in the manufacture of clocks and navigational instruments.

It is interesting, however, that all this new skill at sea did not immediately make the warship more effective. Eighteenth century sailors insisted upon fighting in line formation; they volleyed at each other from a distance but rarely engaged in battles that decided anything. The important role of the warship was to convoy their own commercial vessels or to raid those of the enemy. Not until the end of the eighteenth century did sailors work out naval tactics that would allow an attacking fleet to bring overwhelming firepower against part of an enemy line and thereby destroy it.

My own interests and researches have led me to other aspects of the problem of seventeenth century warfare. Military operations before about 1675 resembled a chess game in the latter stages of play, when the pieces are scattered pell-mell over the entire board and the kings may even be found deep in the middle of the field. Under such a system the rooks—fortifications—may be stationary, but the other pieces are free to move about at will. These pre-1675 armies were commanded by captains who insisted upon autonomy; the conflict between Louvois on the one side and Condé and Turenne on the other, in 1673 and 1674, illustrates the problem. The soldiers often would neither take orders, nor detach troops from their army to support a "rival" commander, even when the king backed up his war ministers. When these great captains were removed by death or retirement, it became possible to establish authority over the marshals in the filed. After 1675, under the direction of Vauban, the French war machine learned a new kind of warfare. Vauban visited Flanders and was horrified to find French and enemy troop encampments and fortifications mixed "pell-mell" all along the frontier. "What your majesty needs is a dueling field (*pré carré*) for a frontier." This *mémoire,* often overlooked and more often not understood by historians, developed a basic conception for both military and foreign policy of the regime. Vauban studded the frontier with strong points; demolished fortifications that could not be easily held; and developed the "lines," that is, fortified fieldworks between the strong points behind which armies could maneuver at will. Out of this came the notion of a lineal frontier. It is the basis for the famous *réunions* that followed the treaty of Nimwegen. Professor Delbrück tells us that the emergence of the uniformed, disciplined armies of the 1680s was indeed the return of the Roman legions to the soil of Europe; I believe that we can also insist that Vauban's *conception* of war and his defense system meant the return of the Roman lines. We see how important this was when Louis XIV's soldiers attempted to defend the Rhineland and the Netherlands in the early years of the War of the Spanish Succession. They had no "lines"

behind which to maneuver, and thus in 1702 Marlborough easily drove Boufflers out of the Rhineland and in 1705 broke through the improvised "lines" that Max and Villory were defending. The war came too soon for the creation of lines such as defended the northern French frontiers.

It is very difficult to know what part technology had in the evolution of this new *conception* of warfare. Perhaps we can insist that the lineal frontier fortification was in fact the product of new engineering technology. In any case the "new warfare" was there to stay. When Marlborough broke through the improvised lines in the Spanish Netherlands in 1705, Louis wondered whether the new practices of his generals suited the genius of the French, whether it would not be better to wage war "as we did in the past, holding the countryside and seeking advantages by making excellent defensive camps . . . ." But there were too many reasons for not going backward, and after 1705 the military logic behind the lines in Flanders proved itself by stopping both Marlborough and Eugene long enough for France to recover from the disasters of 1708–1709.

Up to this point I have not mentioned the processes of warfare that have been most prominent in my own researches in the military archives, perhaps because I am not sure that the terms science or technology are broad enough to cover them. However it is my firm conviction that the most important development in the organization of western European military power was the rise of the ministries of war and marine as bureaucratic organizations responsible for the conduct of warfare. It was characteristic of the older forms of war that either the king himself, or a condottiere commander, took the field and maneuvered as an autonomous power. A Charles V, a Henry IV, a Gustavus Adolphus, a Mansfield, a Wallenstein, or a Bernard von Saxe-Weimar did not have to obey orders from a war minister. They, or the captains that they hired, assumed responsibility for the weapons and training of their troops and the conduct of the campaign. Turenne, who had grown up in this era, bitterly complained when orders came from Paris telling him where he should operate or, even worse, that he must detach troops to assist another commander in a distant field of operations. Indeed he simply ignored such orders. But after the mid-seventeenth century it became increasingly common for the ministry of war to control the actions of commanders in the field. The process started with Richelieu and Sublet de Noyers, Mazarin and Le Tellier, Louis XIV and the war ministry that he inherited. By 1680 the French war ministry was staffed by clerks, engineers, map makers, administrators, soldiers, and even men whom we might see as the prototype of the scientist. The ministry assumed responsibility for a large fan of activities that had formerly been in the province of the condottiere captain: arms, clothing, medical

attention, training, recruitment, supplies of fodder and food, horses and carts, siege equipment, and many other things. Many of the improvements that I have noted in the cannon and the fusil, as well as other developments in the organization of armies, can be traced to experiments carried on by the ministry of war or marine. These war ministers also came to control the behavior of the commanders in the field. When a Le Tellier or his son Louvois was confronted by a Turenne or a Condé whom they could not control by a letter, they simply reduced the number of men assigned to his command. But the future was to see the new type of commander who would be responsible to the orders of his king and the king's minister.

Once armies and navies came to be organized, trained, and directed by the ministries under civilian control, a new orientation became possible. It might be hard to document in detail the relationship between the military reforms of the eighteenth century, which finally developed the weapons and tactics of the Napoleonic era, and the intellectual climate of the Enlightenment; but no one will miss the fact that the introduction of specialized schools for soldiers and sailors, the experiments with powder, guns, harness, etc., the introduction of naval engineers into the shipyards, the adoption of new methods of setting sails and fixing longitude are all in the spirit of this interesting period. Nor will anyone miss the fact that these reforms were usually initiated in the ministry of war or marine. In our own day we see the difficulty that confronts the development of autonomous military power in societies that have not been able to organize effective bureaucratic institutions to direct military effort. It is possible to train soldiers to use sophisticated weapons; but without an effective war ministry, they cannot be used to great advantage, nor will innovations necessary for continued progress be introduced. Thus, it would seem possible to argue that the emergence of war ministries staffed by engineers and administrators and supported by intendants or commissioners who were attached to armies in the field was, in fact, a technological development of prime importance for the emerging art of war.

These intendants of police, justice, and finance who represented the war minister in the field came to have a most important role in the emerging military establishments. One of my students is at present engaged in an investigation of the origins of this office in France. As long as the king led his armies in person, his chancellor and superintendent of finance usually accompanied him; but when the king remained in his capital, he needed a representative to superintend justice and the payment of the troops. As this officer became the representative of the war minister, his functions multiplied in all directions. He not only administered and organized the movement of men and materials, but

also acted as the eyes and ears of the king's government. Many of the innovations in the ways of handling fodder, munitions, and arms came from these men rather than the soldiers who used them.

If these remarks have wandered somewhat far from a discussion of science and technology, perhaps I can justify them by recalling that soldiers must be convinced of the value of their weapons if they are to function effectively, but this fact makes them conservative and unwilling to try innovations. At the opening of the sixteenth century when the different methods of fighting that had emerged independently in Spain, France, Switzerland, Italy, and Germany were tested against each other in the so-called Italian Wars, one might have expected that all armies would immediately adopt the best aspects of each, or at least would attempt to do so. This in fact did not happen perhaps because the captains, rather than a central authority, were responsible for the arming and training of the troops. It took almost a century before European armies digested the problems posed by the confrontations of the Italian wars. However when the organization, training, and arming of troops and the maintenance of warships and sailors became the function of ministers of war and marine, the process of adoption as well as innovation was speeded up considerably so that we might insist that the military revolution that brought back the Roman armies to Europe's soil and Roman lines to the frontiers was primarily the result of the new techniques for organizing and directing the military establishments; and this was, in fact, the product of a new technology.

Discussion

THE CHAIRMAN (Professor LYNN WHITE, JR., University of California at Los Angeles) : Gentlemen, as interlocutor in this show I have vowed—not only have I vowed, I have been instructed—to terminate it at 11:45, which gives us not quite 15 minutes for general discussion. We have an end man here, Doctor Theodore Ropp of the Department of History, Duke University, who is going to add a new voice.

Professor THEODORE ROPP, Duke University: At this very late hour they always introduce the court fool; but I have a number of questions that I want us to discuss, because this session is a commercial for the successive programs here. Notice our banner [above the platform]: "Science, Technology, and Warfare." We have fairly well proved that in fact science, technology, and warfare had almost no connection in the period covered by the three distinguished authorities. Yet by the time that we end, the symposium will get to the famous—or notorious—USAFM 1–1, which says: "Technological and practical improvements must be continuous." I will pass up the problem of whether soldiers must really understand science or technology to have this touching faith. But there are three questions we ought to answer in the next couple of days. First, to direct to these gentlemen here: what do you call the preconditions of the technological revolution that certainly existed from about 1650 onward? Thereafter, there were more craftsmen and more organizers using science to solve technological problems. What terms would you experts use? The second question is: who provided the innovative force? I would contend with Professor Wolf that neither the scientist nor the technologist nor the soldier has been the primary innovating force for the past two centuries, but that it has been Professor Wolf's administrator, beginning perhaps with Richelieu and Carnot, going on to the greatest of the later Carnots, Robert S. McNamara, who must face the problems of organizing the resources of the state for war. The third question is when? It will be perfectly clear, I think, from this afternoon's session that when we get up to 1860 we still do not have anything that in fact could be called what McNeill calls the institutionalization of deliberate innovation in the technological field. By the time we get to Professor Holley's paper and 1945, this phenomenon clearly appears. We have got to decide, I think, when the technological revolution occurred; who were the primary innovators; and what we call

this transition period, because between 1650 and 1850 there was a profound change in the attitude of some soldiers, some administrators, some craftsmen toward technology.

Professor HALE: Can I say something very briefly? I am sure these questions are on the right lines, and I am sure the answer lies partly with the administrator, and I would toss that to Professor Wolf. What I am looking forward to finding out in the sessions which succeed this, is how far there was a correlation between the actual knowledge of the fighting man and the application of scientifically influenced technology to war. I recently read what I think is a fascinating biography of Admiral Fitzroy, who was the man in charge of *Beagle* when Darwin went on that surveying voyage to Patagonia and the Galapagos Islands, and who later became the head of the first Met. office in England. Now Fitzroy went as a career seaman to what passed then for a college for naval officers, and he took a lot of science, advanced astronomy, advanced mathematics, and so on. Now I have looked at some of the textbooks of this academy—they were elementary to a fault. It was impossible for a boy leaving that establishment at 16 to have the faintest idea of how a scientist thought, or how a scientist could couch a thesis in terms that a technologist could implement and produce something useful for war. And yet what he did derive was an enormous respect for science as such. He became—he followed one (American, alas) Commander Torley—he became the world's authority on weather forecasting. He was, I think, the first man to use this term, forecasting of the weather; and he sent out survey ships to send back weather reports. He used in the very early days the Morse telegraph to receive information quickly. All his techniques were in fact scientific, but his conclusions were unscientific. What you have is the phenomenon of a man who went through an educative process that couldn't have made him into a scientist, but nevertheless lived in an environment that made him take up an occupation, an approach to a problem, meteorology, that can be called scientific. So, how far do we have to wait until the concept of science, as such, is sufficiently invasive of the mind of a pretty ignorant and badly educated man to make the armed forces as a whole responsive to the advances of science?

Professor WOLF: I cannot answer your question, of course; but even if it is not the question I was thinking about, it is an interesting one. I think again that I would go back to the characters that we call the intendants and commissars of the army—they appeared in the 17th century and go into the 18th. These are the lineal descendants of the people who went with the king when the king went out with his army. The king took his chancellor and his treasurer, and he himself directed affairs. The kings directed the marshals as long as they were with the

army. When they left the army and when the armies became so complicated that kings no longer played a role in them, they had to arrange for commissars, for intendants, people like that to represent the war ministry. These people played a tremendously important role. They were intelligent, or many of them were. I followed one around Alsace in one of the wars of Louis Quatorze, and he had to deal with the problems of powder, guns, pontoon boats, fodder—a full series of the problems of military organization that led him to make proposals. Today these proposals seem simplistic indeed. They do not demand science, but they were proposals that technologically inventive people could solve; and I am sure that, if you want to find the pressure of technology as the origin of science affecting warfare and the ways of soldiers, you will find a good deal of it in these royal officials who went with the army. They were usually engineers of the war ministry, or they might be lawyers. Men trained as lawyers and engineers picked up the other parts of the military activity and transmitted the information about it back to the war ministry; as these war ministries became more and more complicated, they engaged engineers and various kinds of people—some of them were soldiers and some were not—to utilize information and develop the ideas that officials proposed. This can be studied. I have a student now in Paris working on the intendants of the 1670s; and I believe it will prove to be a very fruitful field for investigation, for understanding the processes of modern warfare as they develop in the empirical period. That doesn't answer your question, Professor, but it's the best I can do with it.

Professor Ropp: I would like to hear from Professor White. What would he call this infrastructure, or the preconditions for science to affect warfare? It certainly existed.

The Chairman: I think that possibly another element might have been added to the title of this entire conference: "Science, Technology, *Management*, and Warfare." The more I get into technological history, the more I'm convinced that management as an art is just as important as technology for getting things done. Of course, Lewis Mumford has made some rather extreme statements in recent years about this, but in a very real sense government is the fundamental machine. I think that as we get deeper into these matters we shall find that we are faced with both a technological revolution and a managerial revolution, which dovetailed in the most extraordinary way, affecting not only industry but also warfare.

May I just throw in a final breath before the gong rings? In what has been said there have been a number of remarks about propaganda or blarney. The implication is that warfare in the earlier period, and perhaps more recently than we are widely prepared to admit, has been

an empirical activity which has garnished itself—to use Rupert Hall's expression—with the ornaments of borrowed science, mathematics, and the like, so as to give it a little flare—intellectual elegance. As a historian, I have great respect for propaganda, for blarney: it has been one of the basic forces shaping history. Rupert Hall took a couple of cracks at an old friend of mine, Edgar Zilsel. I knew him pretty well just before his death. Zilsel was a Viennese Marxist of the old school and, of course, he gave everything an economic-social interpretation, this being the Marxist orthodoxy. Zilsel's best work, which has never appeared in English and is almost forgotten, is *Die Entstehung des Geniebegriffes, The Origin of the Concept of Genius.* His hypothesis was that "genius" is a put-on invented by artists, musicians, literary people, and maybe some architects, a desperate effort to raise their social status above that of the hired hands. And you know, it worked damned well, didn't it? Now I wonder if a similar embellishment of propaganda may not in fact have softened up the empirical military mind through the generations to the point where finally, maybe in the thirties and forties of our own century, military men were willing to take their own blarney seriously and really go in for science. I think that cultural softening-up processes sometimes take generations to become operative. Maybe we shouldn't just mark off the blarney and propaganda as nonsense. On this happy thought I shall adjourn us for lunch.

The Second Session

THE IMPACT OF SCIENCE/TECHNOLOGY ON MILITARY INSTITUTIONS, 1700-1850

MILITARY EDUCATION IN 18TH CENTURY FRANCE; TECHNICAL AND NON-TECHNICAL DETERMINANTS*

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When Louis XV created the *École militaire* in January 1751, none could foresee that it would produce generals and marshals who were to lead French armies across Europe, and that one especially obscure student there would be Emperor of France. But some observers and actors in the 1750s did expect great things of the new institution. In particular, they hoped that a new form of education, designed to be specifically military, would raise up in all ranks a generation of officers whose competence and devotion to duty might restore the effectiveness and reputation of French arms, a reputation that was sagging badly after the stalemate in which the War of the Austrian Succession ended.

The architects of the new education, though differing among themselves over details, could agree on many things. The one central idea was that the formation of officers should be vocational and "technical." The technical, as they saw it, involved serious and long studies in the subjects useful for war, and especially in mathematics. The purpose of this paper will be to explore the meaning of this new and technical formation by asking in what sense it was new and why it seemed so important to the men who founded and ran the *École militaire*.

The study of mathematics by scholars was of course very old, but the notion that young men should ordinarily and routinely apply themselves to it was unknown before the eighteenth century. Earlier, the very young were supposed to know how to count and perhaps also a few elementary rules of arithmetic, but the mark of the educated man was literary knowledge, always including skill in Latin, sometimes Greek,

* Although I have not tried to list here the scattered references, I base much of this paper on data and impressions derived from sources for the *École militaire* in Series M and MM, Archives Nationales, Paris, and in series Yb, Archives du Ministère de la Guerre, Vincennes.

and occasionally Hebrew. Humanism, as codified in the Jesuits' *Ratio Studiorum* and imposed on tens of thousands of young men who flocked to the *collèges* that were springing up everywhere in the late sixteenth and seventeenth centuries, demanded at its best rigorous training in words, expression, and style. Coherence and the capacity to persuade by marshalling logical and pleasing arguments were what mattered most. A literary formalism, a little history and geography, and no mathematics were the distinguishing marks of the liberal, universal, and explicitly non-professional education of the day.

Until the mid-eighteenth century, the interest in mathematics survived and developed in two ways, both outside the educational mainstream and general culture. In the Jesuit *collèges* some professors of philosophy, unenthusiastic at first about doing it and mainly interested themselves in theology, were required to spend at least some of their time in teaching math to very advanced students. In 1627 in the Jesuits' province of Paris, 64 students, having finished their five or six years of rhetoric and a year of logic, were now applying themselves for five hours a week to the mysteries of what was then higher mathematics—geometry, algebra, and sometimes beyond. Some of these students—Descartes, for example—became great mathematicians. But it is worth noting that in 1627, among the 12,565 young men studying with the Jesuits in the Paris province, only one in fourteen of the eligible advanced students, mainly apprentice Jesuits, and but one in 200 of the students at all levels were studying any math at all.[1] This situation changed very slowly, and even in the 1760s Père Navarre, a professor at Toulouse, was proposing a new scheme for a national education in which literature alone occupied the first five years of study.[2] The teaching of math in *collèges* expanded as the number of chairs in the subject proliferated, but this teaching pertained always to the few, to the scholars and their rarefied world of pure and abstract math.

Elsewhere there was genuine development of a technical and mathematical culture. This came from the task-oriented and practical training designed to satisfy specific needs of individuals and state. In the sixteenth century, commercial arithmetic manuals appeared. Drawing on the world of business for practical illustrations, these works instructed aspiring merchants and bankers in fractions, proportionality, extraction

[1] Le R. P. F. de Dainville, S. J., "L'enseignement des mathématiques au XVII^{e} siècle," *XVII^{e} siècle*, no. 30 (Jan. 1956) : p. 64.

[2] Le P. Jean Navarre, *Discours qui a remporté le prix, par le jugement de l'Académie des jeux floraux, sur ces paroles: Quel serait en France le plan d'études le plus avantageux?* [1763]. For this and other references to Père Navarre's work, I am indebted to Mr. Daniel Willbach, candidate for the doctorate in History, University of Michigan, and the research he conducted for a seminar paper on the Robe nobility and Enlightenment at Toulouse.

of roots, and equations, all for the purpose of understanding and calculating profits and losses in partnerships, interest rates, and fluctuations in foreign exchange. In the seventeenth century, academies for painting, sculpture, and architecture appeared, and members communicated to one another and to others the mathematical understanding that their professional activities demanded. Richelieu, Mazarin, Colbert, and Louvois did what they could in the seventeenth century to encourage the diffusion of a technical culture in order to stimulate various developments that they thought useful. A school for civil engineers, the famous Service of the *Ponts et Chaussées,* dated from the 1720s. The state in many localities began to recruit sons of notaries and peasants who had the aptitude and inclination to be surveyors, and paid for their training.[3]

Slowly, from 1650 to 1750, and after many false starts, the government also developed specialized institutions for military training. The navy and, in the army, the so-called scholarly branches of artillery and engineers, were the favored ones. Rightly or wrongly, it was assumed that aiming and firing cannon on land, as well as at sea, required a knowledge of geometry. The building, attack, and defense of fortifications also demanded mathematical skill. Drawing on Jesuits and whomever else it could find, the state in 1670 established schools to teach naval officers math, hydrography, navigation, and artillery. Although that institution's greatest importance came after it was established at La Fère in the 1720s, an artillery school for the army had been opened as early as 1679. The Engineers' large school, founded in Mézières in 1748, was not their first one.[4]

There is little doubt, then, that mathematical studies—both the "pure" math of the *collèges* and the more mundane math of the practical schools and academies—were well established in France from the seventeenth century on. But when we have said this, we have still not explained the peculiar and special development at the *École militaire* beginning in 1751.

The new school, established at Paris on the initiative of the military administrator and financier, Paris-Duverney, recruited boys, aged eight to eleven, for up to eight years of intensive study. To enter, a boy had to show a noble pedigree of at least four generations on his father's side.

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[3] Natalie Z. Davis, "Sixteenth-century Arithmetics," *Journal of the History of Ideas* 21 (1960): 18–47; Frederick B. Artz, "Les débuts de l'éducation technique en France (1500–1700)," *Revue d'histoire moderne* 12 (1937): 469–519. For the surveyors, Archives départmentales de la Gironde, C3297, *passim.*

[4] There is a considerable literature on these establishments. See Artz, "Débuts de l'éducation technique"; bibliographies may also be found in two articles by Roger Hahn, in René Taton, ed., *Enseignement et diffusion des sciences en France au XVIII[e] siècle* (Paris, 1964).

Further, the father had to have had long military service, and the family had to be too poor to provide the son an education. Failure to meet any of these criteria was used systematically as grounds for exclusion. The founders and their successors made no attempt to select boys of especially high intelligence or capacity. The special qualities they were to bring to the new and reformed army would be developed in them by a kind of educational engineering at the school itself. It was hoped and expected that the boys would learn a great deal, but there was also an egalitarian assumption, no doubt correct, that nearly anyone who made the effort could learn what was needed to be a good officer of infantry or cavalry. The administrators were always pleased when the unusually able few turned out to be so adept at math that they could compete successfully, by passing the examinations, for entry into the artillery. But these few were not their main concern. What they wished to produce were professional officers who could fill the lower grades in the non-technical branches.

For this purpose, they designed a new curriculum, one that seemed revolutionary to contemporaries. Paris de Meyzieu, the nephew of the founder and himself in charge of studies at the *École militaire,* described the program and its intent in an article that appeared in 1755 in the great *Encyclopédie.* There he spoke of the need for a vocational education, for training that, by contrast to what the *collèges* offered, would vary in its nature with the profession. He spoke of the risks that were involved in any other education that might end "by making a bishop of a geometer." His readers would catch this reference to the classical education of the *collèges* without difficulty. This *École militaire* would produce only warriors, and he added that he and his colleagues had no intention at all of developing scholars. In the school, the students would study and learn about many subjects: religion, "to the extent suitable for a military man"; French grammar, in order to emphasize understanding and an ability to express oneself easily; Latin, because knowing it made the learning of other foreign languages easier; German and Italian, because these were the languages of the regions where wars would be fought; geography, when it was useful by informing young men about the terrain of likely theatres of war; history, where "one finds examples of virtue, courage, prudence, greatness of soul, attachment to the sovereign . . ."; a little natural law; military ordinances, drill, handling weapons, and in the last year, tactics. Physical exercise was also important.[5]

The most important single study, however, was none of these. It was mathematics. Paris de Meyzieu put it simply: "Among all the kinds

[5] "École militaire," *Encyclopédie,* 1755 ed., 5: 307–13.

of knowledge necessary for military men, mathematics doubtless holds the highest rank." [6] In saying this, he did not mean all kinds of math. He said that geometry was today important, but that the wide interest in "transcendant and sublime geometry," an interest shared even by persons in the literary world, was less respectable in itself than because of the genius of the persons who cultivated it. It was better simply to admire geometry than to care much about it, and military men did not need to know how to calculate the course of a comet anyway. Paris de Meyzieu did speak admiringly of progress made in technical studies in artillery schools, and noted that the *École militaire* would teach this practical math that could have specific applications.

But his main emphasis was on something else. In discussing what was at that moment a controversial decision, to have the students at the *École militaire* study algebra before geometry, the director of studies summarized an argument that was taking place in the army and at the school, one in which both sides shared a single idea: mathematics is important because it trains the mind and forms the judgment. Those who defended geometry argued that it was useful because it has at its base only truth, requires evidence, and accustoms the mind to demonstration, and "demonstration is the end that reasoning proposes to itself." The partisans of geometry thought the study of their subject essential for shaping the young: "To speak only with *justesse,* to judge only by relationships combined with as much exactitude as precision, doubtless is an advantage that cannot be acquired too early . . . ." Against this, Paris de Meyzieu said only that, although geometry might be useful in the sense that its supporters claimed, it was not so useful as algebra. His opponents, in his view, were confusing geometry with the geometric method, and should remember that even quite profound geometers easily went astray on subjects foreign to their specialty. In any event, whether it was geometry or algebra, and eventually both, the two sides shared the conviction that students needed math neither for its practical applications and uses exclusively nor, certainly, for becoming scholars and pushing back the frontiers of knowledge. Math formed the mind; supplemented by some logic stressing clarity of definitions, it was useful for everyone, the slow quite as much as the quick-witted. To succeed in giving a child clear ideas, all that was needed was to build into him the right habits. Reasoned, clear, and certain judgments would flow from a good dose of math.[7]

And it was a good dose. From the 1750s the boys spent one half of the morning, six days a week, working their way toward the upper

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[6] *Ibid.*, p. 310.
[7] *Ibid.*

reaches of geometry and trigonometry. In the 1770s, when the Paris school had the young men only after age fourteen, 16 of the 31 professors were teachers of math. How much the students profited from all this is unclear—the comte de Vaublanc later remembered that in his math classes at the school in the early 1770s, only four or five of the fifty students in attendance had been interested in what was going on and had done well. And yet these results shocked the administrators less than Vaublanc thought: they often observed in their deliberations at meetings of the *École militaire's* Administrative Council that zeal and good will in even mediocre or poor students would yield very good results.[8]

At this point, we can return to the original question—why did this new institution implement a revolution in pedagogy by stressing math for *all* future officers—and try to treat it in different terms. As the administrators of the school and other army reformers saw the problem of the army in mid-century, the crisis in the military was in large part one of subordination, or insubordination, among officers. A new kind of officer was needed, one who understood his trade enough to know that in battles fought in linear formations the essential quality was attention to duty, constant training, and habitual response to fixed situations. Shifting from column to line, although theoretically not difficult, could be managed well under fire only through constant practice. Officers at all levels had to know what they were doing, and they had to be willing to spend time teaching and training the soldiers in the stylized and intricate formations. This need was not new in the mid-eighteenth century, for the formations and tactics were essentially those of Louis XIV's day. But other armies were now better than they had been, and in war mere stalemate at enormous cost was coming to seem frustrating and unworthy to a generation that had not been mellowed by having seen foreign troops on French soil.

When reformers looked at their army and especially the officer corps, they were unhappy. As they saw it, men of wealth bought captaincies and paid little attention to their companies. The rate of turnover among subaltern officers was very high. Officers all down the line wished to discuss and to negotiate the orders they received. In the registers containing reports on the performance of individual officers in the regiments, one of the most common derogatory remarks is the accusation of being a *raisonneur.* Independence, lack of subordination, and this desire to reason over orders seemed a curse. Officers also spoke much of honor, but reformers objected that too often they confused honor with

[8] Comte de Vaublanc, *Souvenirs,* cited by Robert Laulan, "À propos de la formation scolaire de Bonaparte: ce qu'était l'enseignement à l'École militaire de Paris," *Revue des travaux de l'Académie des Sciences Morales et Politiques,* année 1957, 1[er] semestre, p. 179.

simple bravery. They were ready to die, and did in fact get themselves killed in large numbers, but often uselessly because they did not have the right habits and knowledge. A new sense of honor, more mundane but more useful, had to be developed, one that would be professional and would rest on hard work more than simple heroism.

But how had this old officer corps been recruited? It was composed partly of uneducated rural nobles who arrived in their regiments sometimes ready to learn, sometimes not, but in any case never to find anyone there to instruct them. The larger number had the money to buy commissions and to live well; they had also been to school. Most had attended a *collège* and thus could write poems, declaim, and read Latin. They could be witty and clever. Theirs was a world of culture, money, and cities, and to this world they kept ties and returned whenever they felt like it. They absented themselves freely, often at times when the military professionals thought that they should have been training their troops and learning their military functions, sometimes even during war. These were the men raised by Père Navarre, and others like him, who would insist that boys learn Latin before French, and math almost not at all.

What began to appear was the formation within the Enlightenment of two mutually hostile cultures, each of which misunderstood the other. In 1761 Père Navarre at Toulouse, just one year before he won the literary academy's prize for the essay on education, won another prize for his analysis of how the *esprit de système* and *esprit géométrique* contributed to despotism. In his view, belles-lettres or literature was the bastion of freedom and liberties against the incursions of a tyranny that was embedded in a geometric world of quantity.[9] For him, as for many, it did not seem accidental that it was Vauban, the military engineer and fortress builder, who had toward 1700 designed the plan to tax equally the produce of all land in France. Vauban the army officer and quantifier appeared to many the perfect expression of a mentality that, in destroying tax exemptions for privileged persons and classes, would destroy also qualitative distinctions between individuals and corporate groups that were the essence of their liberties. The popular Montesquieu and others, including the military reformer Guibert when he was in his early salon and literary phase, invoked the lessons of the Roman Republic in favor of a citizen army, full of enthusiasm and patriotism, but amateur, an army that all kinds of nobles, not just professionals steeped in math, might participate in intermittently. President Bouhier at Grenoble, like Montesquieu a *parlementaire,* was speaking for the world of humanities, liberty, and constitutional restrictions on the

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[9] *Recueil de l'Académie des jeux floraux* (Toulouse, 1762).

absolute state when in the first half of the eighteenth century he described geometry, astronomy, and physics as:

> a rather vain amusement; all these sterile and fruitless kinds of knowledge are useless in themselves. Men are not born to measure lines, to examine the relationships of angles, and to use all their time in considering the various movements of matter. Their minds are too great, their lives too short, their time too precious for concerning themselves with such little objects; instead men are obliged to be just, equitable, judicious, reasonable in all their discourse, in all their actions, and in all the affairs that they handle; it is these things that they must particularly practice and to which they must form themselves.[10]

Against the offensive by constitutionalists, Robe nobility, the latter-day humanists an offensive for which Montesquieu was the spokesman—the military world, no less aristocratic than the other, began to define itself in increasingly closed and professional terms. For reformist military men, the term *homme de lettres* was an epithet. In 1760 the Council of the *École militaire* agreed with the Minister of War that this literary type represented the very principle of egoism. To them, the education that produced both bishops and Voltaire, *dévots* as well as skeptics, fanatical monks and clever, pleasure-oriented, self-indulgent courtiers, bourgeois *gentilshommes,* and writers, this education was necessarily at fault. In the Enlightenment everything about men and their behavior was explained by the formative environment; and the education they received, together with rearing in the family, was the central issue. If education was general and literary, if it did not train young men for a specific function and emphasize duty, why be surprised that it turned out fops? And when the army was staffed by wealthy, irresponsible, poetry-writing young officers fresh from the *collèges,* what hope was there for the military regeneration that alone could protect France physically and perhaps even save it morally? The view of the *collèges* held by military reformers was of course a caricature of the reality, but the view was no less effective and strongly held because of that.

This brings us back to the question of algebra. In fact, if the leaders at the *École militaire* had carefully read the programs for the teaching of rhetoric, they would have found them not very different from their own. The traditional emphasis in rhetoric—an emphasis then being renewed by reformers in education—was on order, clarity, and precision in thought, on training the mind in order to make clear definition and careful analysis a matter of habit. These were exactly the qualities that the military school was trying to build through the study of algebra! Rhetoric could be pedantic or frivolous, it is true, but so could math, as

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[10] Dainville, "L'enseignement des mathématiques dans les collèges jésuites de France du XVI^{e} au XVIII^{e} siècle," *Revue d'histoire des sciences et de leurs applications* 7 (1954): 15.

the reformers of the army themselves recognized. Given the aims and purposes of the army administrators, rhetoric and logic might have made a perfectly reasonable curriculum for infantry and cavalry lieutenants. The army was thought simply to need serious officers, ones who could reason clearly and who would devote themselves to work. Nothing that most officers did required more than a little arithmetic, and certainly not algebra. The materials for revising and reforming the education based on rhetoric were everywhere at hand, but the *École militaire* elected not to use them. Why?

For the answer to this question, we need to look, I think, into the political, social, and moral spheres as much as into the technical. The aristocratic, sovereign courts, or *Parlements,* and the Church as well, were filled by persons steeped in Latin, rhetoric, and literature; they were also the institutions that seemed to be blocking reform in the state and blocking the establishment of a rational tax base that could support a better army where rich young officers could not buy their way in. The amateur and dilettante officer had usually been educated in a *collège.* Literature seemed light and frivolous, not at all consistent with the rising secular Puritanism of army reformers. The solution to these problems seemed to be to seal the army off from a society that looked to them corrupt, to make the army self-contained, a separate world. Into the *École militaire,* as into the whole officer corps after 1781, would be recruited the sons of old noble families whose fathers and ancestors were, by their long military service, within the guild. Ideally these sons should also be poor so that, lacking an independent base in influential relatives and lacking alternatives to long and serious professional service, they could be formed into what the army believed it needed. Then, when it came to molding this human raw material through education, it seemed appropriate to impose on it the new and separate culture of mathematics, an intellectual culture that would distinguish and remove the young officers from the larger culture that was seen to threaten the nation's military health and power.

If the decision for math, then, had to do with practical needs, these needs were not the kind that we usually define as technical. Officers would not use their math much, and by the 1780s some were even reacting against it as stultifying and destructive of initiative in the individual officer. Perhaps the emphasis on mathematics helped to create a new military culture within which problems could be defined differently; perhaps there were technical and even technological *results* that came from the new education. But to explain how the new "technical" education for all officers appeared and spread, we will do better to look into the social and moral dimensions to the problem.

Commentary

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Western Military Education, 1700–1850

Anyone familiar with the curriculum of American service academies will recognize the historical importance of Professor Bien's subject: the role of mathematics in the formative period of military education. The long-avowed intention of West Point to "train the powers of analysis" of a future officer so that "his mind may reason to a logical conclusion" has always depended heavily on the special intellectual qualities which the study of algebra, geometry, trigonometry, and calculus are supposed to impart. West Point's historic emphasis on engineering subjects, for future line officers as well as military engineers, has had not only a strictly practical purpose but also the recognized function of keeping mathematical skills, acquired early in a four-year course, well lubricated until graduation. But to note the resemblance between the early *École militaire* and modern West Point is simply to point up several questions implicit in Bien's essay, questions that need answers if we are to measure the validity and the significance of his argument.

Whatever the inadequacy of the brief discussion that follows, the questions themselves are worth serious attention from anyone who would trace the interaction of scientific and technological change on the one hand with the development of warfare and the military profession on the other. The questions are as follows:

1. Can changes in science and technology alone account for the stress on mathematics in military education from the middle of the eighteenth century?
2. Do other contemporary Western societies reveal changes in military education comparable to those at the *École militaire* in France?
3. How was this aspect of the development of the military profession affected by the great revolution in warfare at the end of the eighteenth century?
4. What were the enduring effects, if any, in the nineteenth century of the concept of military education first given institutional form at the *École militaire* in 1751?

The answer to the first question, concerning the pressure exerted on military education by scientific and technological change, appears surprisingly negative, or at least much less simple than textbook versions of the history of warfare and technology suggest. Certainly the scientific revolution of the seventeenth century continued to unfold in the eighteenth century, and the so-called scientific branches of armed force—artillery and engineers—steadily became full-fledged components of military power. Whether we think of the perfection by Vauban of a new science of fortification and siege warfare during the reign of Louis XIV, the rationalization by Gribeauval of field artillery after the Seven Years' War, or the establishment in Austria, England, and France of schools of military engineering prior to the founding of the *École militaire* itself, we see that the technical side of warfare was being dealt with in an increasingly systematic way.[1] Seen in a broader perspective, however, the period from the early eighteenth century until almost the middle of the nineteenth century is remarkable for the very *slow* rate of change in military technology. With the development and general acceptance by about 1700 of a cheap, reliable infantry firearm which, when equipped with a ring bayonet, could also protect infantry against cavalry attack, the tools of land warfare acquired a stability that they would retain until the technological breakthrough of the 1840s.

Long after Waterloo, courses on permanent fortification still began and virtually ended with the study of Vauban, while the steady improvement of artillery was insufficient to change, even under Napoleon, the subordinate role which it, like cavalry, played on the battlefield.[2]

Only with the almost simultaneous appearance just before 1850 of practical rifled weapons, an extensive rail system, efficient steam propulsion for ships, and electrical communications, did technological change begin to force major changes in the nature of warfare. Of course invention occurred in most cases much earlier, but we are speaking here of the development, general acceptance, and deployment that can explain the timing of related changes. In a real sense, then, European

[1] Austria 1717, Great Britain 1741, and France 1748.

*Professor Peter Paret of Stanford University gave me the benefit of his criticism when I revised this comment for publication.

[2] Here I reject the frequently expressed argument, for example in Richard Glover, *Peninsular Preparation: The Reform of the British Army, 1795–1809* (Cambridge, 1963), pp. 68, 83 ff., concerning the decisive effect of artillery in the Napoleonic wars, and accept the results of more intensive study in Matti Lauerma, *L'Artillerie de campagne française pendant les guerres de la Révolution* (Helsinki, 1956), who distinguishes carefully between important changes in organization and tactics, and the decisive effects of those changes; the former indeed took place during the Revolutionary and Napoleonic wars, but the latter appear only much later.

warfare, when compared to both earlier and later periods, existed for well over a century on a technological plateau.[3]

The value of recognizing this plateau is in the support it gives to Bien's argument: technological pressures were not great enough to shape military education in eighteenth-century France, at least not for the more numerous officers who served in the line—infantry and cavalry. While the value of the line officer knowing sufficient math to calculate a march table, to deploy a large formation on the battlefield, or to work more intelligently with gunners and engineers was obvious, the special virtue of mathematics for the line officer, the virtue that justified its privileged place in the new professional curriculum, had less to do with the purely technical aspects of eighteenth-century warfare than might at first glance seem to be the case. On the contrary, mere military technique was regarded as an improving yet stable, reasonably finite, and fairly well-known if often misunderstood body of knowledge. Educated officers were desirable, but education mea knowing something about the routine and perhaps the history of the military trade and above all how to act like a gentleman. Controversy might rage over certain technical questions, like linear versus columnar tactics, but the relative stability of technique left military reformers of the *ancien régime* free to define the urgent task of professional education largely in non-technical terms, in terms that are essentially moral and psychological, in terms of attitude, perception, judgment, and character. Technological stability cannot by itself explain the educational and professional changes described by Bien, but it is difficult to imagine the new curriculum being designed as it was if the tools and techniques of warfare had in fact been undergoing rapid change.

When we ask the second question, whether changes in military education like those at the *École militaire* were taking place outside France, the evidence again points to a negative conclusion. Frederick the Great displayed an interest after the Seven Years' War in raising the educational level of the Prussian officer corps, but he did not put any novel stress on the alleged military value of mathematics, aside from its application to gunnery and engineering. Neither the small Academy of Nobles, organized in 1764 and sometimes called the "*École militaire*" by Frederick, nor the regional military academies, organized late in his reign and attended by two young officers per regiment from

[3] Professor Hughes does not agree, so readers will have to decide for themselves whether changes in the tools of war in the eighteenth century can account for changes in military education, in France and elsewhere. Some of our disagreement may arise from his concern with the long-run effects of certain technological changes on technology itself, while mine is with the immediate effects on warfare and the military profession.

November to February over a three year period, gave mathematics a special place. The Academy of Nobles, a six-year course especially for promising boys from impoverished families, actually incorporated something like the literary concept of education that the *École militaire* had explicitly rejected. The regional military academies taught a little math to young infantry officers, but only in order to enable them to grasp the rudiments of fortification; Frederick excused cavalry officers from mathematical instruction because he said they had no need for that kind of knowledge. Hermann von Boyen—the future colleague of Scharnhorst, Gneisenau, and Clausewitz—attended the regional academy at Königsberg when he was sixteen, and learned most from hearing lectures in philosophy, history, economics, and natural science at the nearby university—a practice expressly encouraged by the regulations governing these loosely organized military schools. So there too the more general literary, rather than the specifically mathematical, concept of military education held sway.[4]

Nothing in British, Austrian, or Russian military history seems to contradict the impression that the curriculum of the *École militaire* was peculiar to eighteenth-century France. Both Maria Theresa and Catherine the Great carried through extensive military reforms, but apparently without giving mathematics any unusual role in the intellectual formation of their respective officer corps. In Britain the Duke of Richmond proposed in 1788 to form a national military academy to train future officers of the line, but the idea aroused general anti-military opposition and was quickly dropped, so at most we can say that the role mathematics might have played in such an institution remains unclear. In France itself there were those, like the author of *De l'esprit militaire,* who rejected any sort of institutionalized education for future officers in favor of the "natural" education which could best be provided by a wise and loving family and by the unrestricted freedom of observing and riding over the countryside.[5] In answering the second question, then, we again find support for Bien's argument: the educational concept of the *École militaire* was in its time less a broad European development than it was an idea held by one sector of French opinion.

The third question, about the effect of the French Revolution and the Napoleonic wars on the concept of a math-centered military education, is more difficult to answer. Few of the officers who had been

[4] Bernhard von Poten, *Geschichte des Militär-Erziehungs- und Bildungswesen in den Landen deutscher Zunge,* Vol. 17 in *Monumenta Germaniae Paedagogica* (Berlin, 1893), pp. 26 and 130. Friedrich Meinecke, *Das Leben des Generalfeldmarschalls Hermann von Boyen* (Stuttgart, 1896–99), 1: 24–32.

[5] Third edition, Paris, 1789.

exposed to such an education at the *École militaire* played a positive role in the Revolution; some were too old, most were aristocrats, and many retired or emigrated. Georges Six has calculated that of the 936 Revolutionary and Napoleonic general officers who had been members of the Royal Army, 237 had some formal military education; but of this 237, only 21 had attended the *École militaire* at Paris. More than twice as many had attended the engineer school at Mézières, and almost three times as many had been to one of the artillery schools. Taking the whole group of more than 2,200 Revolutionary and Napoleonic generals, we might even conclude that a great breakthrough in warfare was achieved, not by school-trained officers, but by the "natural" soldiers—the leaders with a keen sense of terrain, an easy rapport with their horses and their men, and a zest for life, country, and especially battle—the very type extolled by the author of *De l'esprit militaire*.[6] But the fact is that the Revolutionary government, and then Napoleon himself, reformed, expanded, and consolidated a system of military education that would endure, be widely imitated, and eventually give to mathematics much the same role it had had in the original *École militaire*.

Neither the short-lived *École de Mars* (1793) nor the *École speciale militaire*, established at Fontainebleau in 1803 and moved to Saint-Cyr in 1808, avowedly stressed the intellectual and disciplinary virtues of mathematics. Both operated under the severe pressures of war and tried above all to produce the maximum number of reasonably competent subalterns of infantry and cavalry in the shortest possible time. The nominal course at Saint-Cyr was two years, but hundreds passed only a few months at the school before being posted to regiments. Yet the early Saint-Cyr, for all its stress on practical military training, did have an academic program, and in that program mathematics took first place. Napoleon did not expect intellectually polished young officers from Saint-Cyr, but he wanted more than the courageous animals he could easily find in the ranks of the *Grande Armée:* "Cadets must join their regiments knowing more than the old infantry officers; they should know something about mathematics and fortification, less about literature." At one time he hoped to make his new *École militaire* the sole source of line officers, but left us to speculate on his exact reasons

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[6] Georges Six, *Les généraux de la Révolution et de l'Empire* (Paris, 1947), pp. 37–45. In 1776 the *École militaire* at Paris was suppressed and regional academies created in its place, but soon after was re-established as the senior school for the most promising cadets, among them of course Napoleon Bonaparte. The Paris school was again closed in 1788, mainly by the efforts of its old enemy, Guibert, and the regional schools were soon abolished by the Revolution as nurseries of aristocracy and counter-revolution.

for wanting such a radical change in the subaltern structure of an army that had conquered Europe. The entrance examination for Saint-Cyr, then and after 1815, in addition to measuring bare literacy was a test of basic mathematical knowledge.

Still more striking is the curriculum of the so-called *prytanées,* which were established in 1801 and eventually became the military preparatory school at La Flèche. Students at the *prytanée* bound for a civilian career studied humanities, rhetoric, and philosophy, while future officers concentrated on algebra, geometry, trigonometry, natural sciences, as well as military drawing and fortification.

Most influential was the *École polytechnique,* established in 1794 to train civil engineers, but at first under the force of circumstances and later by tradition destined to send most of its output to the artillery-engineer school at Metz and into the army. The extraordinary emphasis of the *École polytechnique* on mathematics, beyond any immediate applications to engineering, was well known and became a subject of recurrent controversy throughout the nineteenth century.[7]

It would be wrong to ignore any of the factors operating in this great reform of French military education, especially the influential role played by former engineer and artillery officers, who—like Carnot and Bonaparte—joined the Revolution more readily than did line officers from the Royal Army, and the widely held belief that mathematics and the natural sciences were revolutionary and progressive branches of learning, in contrast to the aristocratic and somewhat decadent aura surrounding belles-lettres and the humanities in general. At the same time, it would be equally wrong not to see the old concept of 1751, that mathematics has a special value for the military profession beyond its direct utility, as one of those operating factors. Surely the concept remained entangled, perhaps inextricably, with the definition of "military science" as ballistics, fortification, and the arithmetic of staff work. But that the concept existed and continued to flourish, however confused it may have been, becomes even more obvious when we look outside France in the Revolutionary and Napoleonic era.

The *École polytechnique,* Saint-Cyr, Sandhurst, West Point, and the reformed General War School at Berlin—all had their origins in a very short period around 1800. Each institution differed from the others in important ways—in size, mission, curriculum, character of discipline, and age-level of students. But the similarities are striking, and these

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[7] Eugène Titeux, *Saint-Cyr* (Paris, 1898), chapters 2, 4, and 5; the quotation is from p. 180. Frederick B. Artz, *The Development of Technical Education in France, 1500–1850* (Cambridge and London, 1966), has valuable sections on military education.

are what draw our attention. First and most obvious is the near simultaneity of founding: both the timing and the specific circumstances make it clear that each institution represents a response to the changes in warfare effected by the French Revolution. Second is the unusual importance each institution attached to the study of mathematics. Much as the emphasis on math at the *École polytechnique* and for entrance to Saint-Cyr influenced the shape of French secondary education throughout the nineteenth century, military reformers in Prussia after the disaster at Jena in 1806 called for greater stress on mathematics in the secondary schools, describing math as the discipline that best develops the powers of judgment. West Point (1802), while sending most of its tiny contingent into line regiments, took the *École polytechnique* as its model, and through the early decades of the nineteenth century West Point was a leading center of mathematical studies in the United States. Organizers of Sandhurst (1802) understandably did not advertise French models to justify their own enterprise, but from the beginning it was understood that mathematics was the measure of academic success and the principal pathway to a commission in a line regiment without purchase. Sandhurst, like the *École polytechnique,* suffered repeated attacks after 1815 for emphasizing mathematics at the expense of either a more practical or a more liberal education for the profession of arms.[8]

Nothing demonstrates more persuasively the international influence of what had originally seemed a peculiarly French concept of military education than the words of the most famous and effective opponents of Napoleon. Scharnhorst spoke, not merely for himself or a small group of Prussian reformers, but for a whole generation of progressive military thinkers, when he wrote even before Jena that only the study of mathematics "imparts a proper feeling for truth and accuracy," or in 1811 justified giving mathematics most of the time in the new *Kriegsschule*

> because it is to be considered not only as the basis for more advanced military

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[8] Standard histories of the various academies bear out the assertions in this paragraph, but only the evidence found in archives and contemporary publications can fully support everything said. On Prussia, Poten, *Militär-Erziehungswesen,* and Gottlieb Friedlaender, *Die Königliche Allgemeine Kriegs-Schule* (Berlin, 1854); on the United States, Stephen E. Ambrose, *Duty, Honor, Country* (Baltimore, 1966); on Britain, Augustus Mockler-Ferryman, *Annals of Sandhurst* (London, 1900). Henry Barnard, *Military Schools,* rev. ed. (New York, 1872) is also valuable. I can leave the case of Austria to Professor Rothenberg, but it is worth noting that plans for the reform of Austrian military education in the 1860s called for an end to "an exclusive mathematical course" (Barnard, p. 453).

> education, but also as the best means of attaining mental acuteness and a clearly logical mode of thought.[9]

Wellington, far more conservative than Scharnhorst and unsympathetic to the idea of making officers in military schools, nevertheless shared the same concept of professional education. "If you should continue in the Army," he wrote to his son in 1826,

> it is absolutely necessary that you should make yourself master of those branches of the Mathematics applicable to the Military Service. . . . There is not a movement of any body of Men however small whether on Horseback or on foot, nor an operation or march of any description nor any Service in the field that is not founded upon some mathematical Principle.[10]

Many expressions by lesser men to the same effect could be quoted.

Although the military and educational history of each society, regarded individually, provides some explanation for a new concern after 1800 with mathematics as the key to military education, only a comparative, international approach can lead to a complete explanation. It may be an oversimplification to say that the original concept of the *École militaire* was revived and reinforced by French Revolutionary leaders and especially by its most famous cadet, and that French military success in turn elicited an imitative response in Prussia, Britain, and the United States; but the oversimplification contains a vital truth.

We have already encroached on the answer to the fourth and last question, concerning long-run effects. As the new military academies relaxed for a half century after 1815, as budgets tightened and governments lost interest, the emphasis on mathematics for its own sake became if anything more pronounced; "secondary" or "minor" subjects were the first to be cut back. Even horsemanship suffered more than math. Only in Prussia, perhaps for reasons peculiar to Prussian society, does the military curriculum appear to have become less math-centered, though there was no question of discarding this intellectual legacy of Scharnhorst altogether.[11] But from here on historical ignorance and speculation have to march hand in hand. We have as yet no clear idea what an early commitment to mathematics did outside the academies, to the military profession as a whole, nor whether national differences, like that just mentioned between Prussia and the other Powers, were consequential. Nor can we do more at this stage than guess about the

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[9] Poten, *Militär-Erziehungswesen*, p. 158. The previous quotation is in Rudolph Stadelmann, *Scharnhorst: Schicksal und geistige Welt* (Wiesbaden, 1952), p. 127.

[10] Duke of Wellington, *A Selection from the Private Correspondence* (London, 1952), pp. 44–46.

[11] Meinecke, *Boyen*, 2: 106–15; Eberhard Kessel, *Moltke* (Stuttgart, 1957), pp. 31ff.

practical results of encouraging an aspiring young officer to think in mathematical terms. Certainly, by giving him a link to the technical or "scientific" branches of the service, the academy prepared him to grasp the nature of major technological changes when they occurred. But possibly more important were those effects of mathematical training that transcend merely practical application to technology; in other words, the very effects predicted and hoped for by Paris de Meyzieu, Scharnhorst, Wellington, and Sylvanus Thayer—that the professional soldier would see his work as a realm of thought and action essentially geometric and algebraic in character. Some such effects surely appear in the writing and teaching of military history in the nineteenth and even the twentieth centuries. But in general we must fall back on the old plea for more light, because no one has really studied the deeper and more lasting impact of mathematical thinking on the military profession. About the significance of the questions raised by Professor Bien in his valuable essay, however, there can be no question at all.

Commentary

Thomas P. Hughes

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Professor Bien has provided a perspective on military education in eighteenth-century France that is thought provoking and that should stimulate other scholars in their research and writing. I look forward to reading more of the results of his research in the archival records of the *École militaire*. In my commentary, I do not intend to challenge the validity of Professor Bien's argument that non-technical factors shaped the course of study at the *École militaire*. Specifically, I shall not question his contention that some of those insisting upon mathematics hoped to establish a culture that would distinguish and remove the younger officers from the corrupting larger culture. I want simply to expand upon two minor theses stated by Professor Bien and then to suggest that there were, in eighteenth-century France, practical, technological reasons—as well as non-technological ones—for stressing math in the curriculum of the *École militaire*.

In his opening paragraph, Professor Bien asks if Louis XV could have foreseen that the *École militaire* would produce generals and marshals who would lead French armies across Europe. I doubt that Louis XV and others interested in establishing the school foresaw the French conquest of Europe, but I do believe—and this helps explain the mathematics and science taught at the *École militaire*—they intended that the school produce generals and marshals. This suggests to me that Louis Antoine Paris, Paris-Duverney, and others, who helped to establish the *École militaire,* saw the need to familiarize all officers with the problem-solving techniques of all branches of the army, including the engineering and artillery. This familiarity seems to have been imperative in an era that valued highly the achievements of military engineers and the potential of artillery. The spirit of Sébastien le Prestre de Vauban (1633–1707), the most famous of the military engineers, still had enormous influence in mid-eighteenth century France.[1]

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* Since delivering this commentary, Dr. Hughes has been appointed Professor of the history of Technology, Southern Methodist University.

[1] Henry Guerlac, "Vauban: The Impact of Science on War," in *Makers of Modern Strategy,* ed. by Edward Mead Earle (Princeton, 1943), pp. 34–35.

Although preparation for the artillery and engineering branches—specialties that required mathematics and science—may not have been a major concern of the administrators of the *École militaire* at the time of the founding, their attitude changed in the next quarter century. French military education became hierarchical, and the school received students from preparatory schools and sent graduates to the advanced artillery school at La Fère and the engineering school at Mézières. The preparatory schools allowed the *École militaire* to raise its standards and emphasize the study of mathematics and physics.[2] Undoubtedly, a reason for this emphasis was a concerned effort to prepare students for the specialty schools—a practical reason related to military technology.

Professor Frederick Artz, a student of French technical education, not only finds this trend toward hierarchy and increased emphasis upon mathematics at the *École militaire,* but he also places more emphasis, than does Professor Bien, upon the desire of the founders to prepare all officers to apply science and mathematics to military engineering. Greatly impressed by the application of scientific discoveries to military engineering and to the uses of artillery as shown in the military treatises of the first half of the eighteenth century, Antoine Paris, who popularized the idea of a military school in court circles, believed that more science in military instruction was necessary.[3]

There were practical reasons, as Antoine Paris recognized, for improving general military education. These reasons were, I believe, based on the success, and promise of success, resulting from the application of science and mathematics to warfare. Professor Bien has argued that the purpose of the mathematics was to create a social and moral culture isolating the officer from the corrupting larger culture of pre-revolutionary France; I also want to suggest that the purpose of that increased emphasis was to initiate the young officer into a culture, the one of practical science and technology. Therefore, in stressing mathematics, the founders of the *École militaire* were motivated by the technological results that they believed would come from the new education.

All officers needed grounding in mathematics and science in order to comprehend the manner in which the military had waged war since the sixteenth century. In mid-eighteenth century, when the *École militaire* was founded, the predominating means of waging war was by fortification and siege. Although the move was toward improved artillery, mobility, and field warfare, nonetheless fortification and siegecraft remained the most important aspect of warfare.[4] Even at the end of

[2] Frederick B. Artz, *The Development of Technical Education in France, 1500–1850* (Cambridge, Mass., 1966), pp. 91, 92, 95, and 99.

[3] *Ibid.*, p. 89.

[4] Guerlac, "Vauban," p. 34.

the century, on the eve of the Napoleonic wars when field warfare became common, the French were developing a new scheme of fortification, the "perpendicular," based on the work of mathematician Gaspard Monge and the military engineer Lazare Carnot.[5] Before this the principles of Vauban had exerted the major influence. At the engineering school at Mézières, established in 1748, the official school of thought was based on Vauban.[6] The drama and the dynamic of the Napoleonic wars should not obscure that when the students at the newly founded *École militaire* were required to study mathematics, it was as a prerequisite for the study of siegecraft, which remained a focus of the education of a military officer—perhaps even of the "education of a gentleman." [7]

It is not necessary to dwell here on the relevance of geometry and mathematical analysis to fortification and siegecraft. Professor Lynn White has written that in fortification as early as the sixteenth century "safety was achieved less by tangible masses of masonry than by abstract geometrical patterns of lines of fire." [8] By then the Italians had perfected the bastioned fortress, and as early as 1557 an Italian author treated the planning and design of fortification as purely abstract and geometrical.[9] There is also no need here to demonstrate that the successes of Vauban in fortification, and especially in siege, greatly enhanced the prestige of *esprit géométrique,* a spirit manifest in the man and his works. Although he flourished in the seventeenth century, his two famous works on fortification and siegecraft were not published until 1737 and 1740.[10] After Vauban, engineers made enormous efforts to improve upon details. In the eighteenth century this refinement involved precise calculation of the amount of sapping—the number of days—necessary to overwhelm a fortification. There was danger that the overly subtle system and analysis would produce impractical theory.[11] In view of the ascendancy of fortification and siegecraft and the efficacy of mathematical analysis when applied to it, at the time the *École militaire* was founded, I believe that it would be more difficult to explain a failure to stress mathematics than to explain the stress on it.

[5] John U. Nef, *Western Civilization since the Renaissance* (New York, 1963), p. 319.

[6] Louis Charles Jackson, "Fortification and Siegecraft," *The Encyclopaedia Britannica* (11th ed.; New York, 1910), p. 688.

[7] Sébastien le Prestre de Vauban, *A Manual of Siegecraft and Fortification,* ed. and trans. by George A. Rothrock (Ann Arbor, 1968), p. v.

[8] Lynn White, "Jacopo Aconcio as an Engineer," *American Historical Review* 72 (1967): 425.

[9] Horst de la Croix, "The Literature on Fortification in Renaissance Italy," *Technology and Culture* 4 (1963):41.

[10] Guerlac, "Vauban," p. 12.

[11] Jackson, "Fortification," p. 688.

Already we have heard that planning and design of fortification and siegecraft were matters of geometrical analysis; now I wish to note the increasing application in the eighteenth century of mathematics and science in the actual construction of military works. This trend was well publicized by important eighteenth-century French treatises on construction, and these probably helped convince the organizers of military education of the desirability of grounding officers in mathematics and science.

In the eighteenth century, military engineers and the engineers of the *ponts et chausées* increasingly used accurate, rationalized, and quantitative techniques to replace the qualitative and intuitive ones of the past.[12] They systematically tried the exact methods of mathematics, geometry, and statics, and performed strength tests to determine the dimensions of structures, retaining walls, and other building elements.[13] Several military engineers published outstanding treatises explaining such methods. One of the first works of this kind, *La Science des ingénieurs* published in 1729 by Bernard Forest de Belidor (1697–1761), was repeatedly reissued until 1830.[14] Belidor was a military engineer and a teacher of mathematics and physics at the Artillery College of La Fère. In 1758 he became Director of the Paris Arsenal and Inspector-General of technical troops. He also contributed books on fortress engineering, and his *Architecture hydraulique* included an exhaustive description of mechanical engineering. The popularity of the military engineer's books are evidence of the existence of a French military culture based on practical science and technology.

Belidor was by no means the only French military engineer who contributed to the rise of scientific, quantitative engineering. A graduate of the engineering school at Mézières, in charge of fortification works at Martinique, Charles Auguste Coulomb (1736–1806) investigated the statical behavior of building elements by approaching the problem mathematically. Though intended for his personal use in planning and supervising engineering works, the results were published in his now famous essay of 1773 (*Essais sur une application des règles de maximis et minimis à quelques problèmes de statique relatifs à*

[12] James Kip Finch, "Hubert Gautier's Roads and Bridges," *Consulting Engineer*, (Oct. 1960), p. 123.

[13] Hans Straub, *A History of Civil Engineering* (Cambridge, Mass., 1964), pp. 120–121.

[14] Bernard Forest de Belidor, *La Sciense des ingénieurs dans la conduite des travaux de fortification et d'architecture civile* (Paris, 1729). Belidor not only wrote on the construction of fortifications in *La Science,* but also on the theory of masonry construction, the mechanics of arches, the construction and decoration of military buildings (gatehouses, foundries, bakeries, powder magazines, officers' quarters, etc.), and on the manner of making building contractor's estimates.

l'architecture). He returned to France in 1776 and earned a reputation for research on mathematics and magnetism.[15]

In this commentary, I can only indicate the quality of French military engineering in the eighteenth and early nineteenth centuries, but in passing I must mention the names of L. Carnot, Borda, Poncelet, and Frézier. It is necessary, however, also to note briefly the role of science in the military culture of France. John Nef has noted the readiness of French eighteenth-century scientists to contribute to the solution of military problems.[16] The case of Gaspard Monge (1746–1818) is unusually interesting. As a young draftsman attached to the military school at Mézières, he was assigned a problem of the *défilement* of a fortress. His solution was so original and practical that he was encouraged to continue his mathematical researches, the result of which was the development of descriptive geometry.[17] The military thought his method so useful that officers instructed in it were forbidden to communicate it, even to those engaged in other branches of public service. Monge became a professor of mathematics and then of physics at Mézières, Minister of Marine from 1792 to 1793, and a founder of the *École polytechnique* in 1795. He also contributed to the development of the metallurgy and manufacture of French artillery at the time of the Revolution.[18]

I have emphasized the concern of the military with the solution of problems that today would be generally classified as military or civil engineering.[19] If time permitted, I could suggest the usefulness of mathematics and science to the improvement of artillery, especially as the eighteenth century drew to a close and the French rose to preeminence in this field.[20] Though a founder of modern gunnery was an Englishman, Benjamin Robins, who in his *New Principles of Gunnery* (1742) placed gunnery on a "scientific footing," the French subsequently made the greatest progress in artillery.[21] This can be attributed

[15] Straub, *History of Civil Engineering*, p. 146.

[16] Nef, *Western Civilization*, pp. 319–321.

[17] On Monge, see P. J. Booker, "Gaspard Monge (1746–1818) and His Effect on Engineering Drawing and Technical Education," *Transactions of the Newcomen Society* (London) 34 (1961–62): 15–36.

[18] Arthur Cayley, "Gaspard Monge," *Encyclopaedia Britannica* (11th ed.: New York, 1911), pp. 709–710.

[19] S. B. Hamilton, "The French Civil Engineers of the Eighteenth Century," *Transactions of the Newcomen Society* 22 (1941–42): 149–59.

[20] Robert Quimby, *The Background of Napoleonic Warfare* (New York, 1957), pp. 3–4.

[21] J. F. C. Fuller, *A Military History of the Western World* (New York, 1955), 2: 350.

in large measure to Jean Baptiste de Gribeauval (1715–1789), who strove after 1765 to rationalize and standardize artillery manufacture.[22]

These developments suggest an interaction between military developments and the rapid improvements in metallurgy and mechanical engineering.

Finally, I should emphasize that the evidence I have offered does not contradict the thesis of Professor Bien, but it does suggest that there were practical scientific and technological reasons for the *Écoles militaires* stressing mathematics and teaching science to all officers, and especially to those who might qualify for the advanced technical schools. I also emphasize that science and technology were an integral and practical part of the military culture of eighteenth century France.

[22] Theodore Ropp, *War in the Modern World* (Durham, N.C., 1959), p. 83.

Commentary

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Some Observations on the Evolution of Technical and Scientific Education in the Austrian Army during the Eighteenth Century

Throughout most of the eighteenth century there was but little technical or scientific education for officers. Although flintlock and bayonet had superseded musket and pike, though artillery and military engineering had been greatly improved, and the disparate corps of the previous century had been replaced by national, or rather dynastic, armies, the weight of tradition, reinforced by political and social considerations, still maintained that the only requirement for command was the inborn courage and honor of the nobility. Nonetheless, the technical arms, that is to say the artillery and the engineers, required technically competent officers; and before the end of the century almost all European states had established schools to provide the necessary training.

Austria was no exception. During this period, except for Joseph II, the Habsburg rulers showed little enthusiasm for professional military education and scholarship. Ruling over an empire lacking geographic, national, and political cohesion and united only in the person of the ruler, they conceived their ideal officer not as a learned man, but as an aristocratic soldier devoted and loyal to the dynasty. When in 1752 Maria Theresa founded a new military academy, later called the *Theresianische Militärakademie,* in Vienna-Neustadt, she instructed its first superintendent to produce not scholars, "but brave, loyal, and chivalrous men." And one hundred years later, in 1850, Francis Joseph made a very similar statement. "The strength of my army," the emperor wrote, "does not depend on learned officers, but on brave and chivalrous men." If despite such attitudes the Austrian service developed specialized, and in the case of the artillery extremely progressive, technical training establishments, the reason can be found in empirical military necessity.

The standing army of the Habsburgs came into being at the end

of the Thirty Years' War. After 1650, repeatedly engaged in campaigns against the Turks and French, it rapidly grew in size, numbering some 100,000 effectives during the War of the Spanish Succession. Apart from its special frontier defense establishment against the Turks, the army was similar to those of other western and central European states. Its main fighting elements were foot and horse; gunners and engineers were present only in very small numbers. In the two primary fighting branches the officers remained essentially amateurs, learning their profession by doing and by studying available military manuals. The outstanding Austrian commanders of this period, Raimondo Count Montecuccoli, Charles of Lorraine, and Eugene of Savoy, had no formal military education though they appreciated the need for such in the three technological branches of the service—staff, artillery, and engineers.

During the eighteenth century, however, only the artillery and the engineers developed formal training establishments. Staff officers still were regarded primarily as personal assistants to the commander; and noble birth and connections, rather than ability or formal training, continued to be the criteria for their selection. In any case, these personal staffs remained rudimentary and were formed on an ad hoc basis for each campaign. Although after the conclusion of the Seven Years' War regulations for operational staff duties, formulated by Fieldmarshal Count Lacy, were issued in 1769, the Austrian Quartermaster General Staff, as it was called until 1863, did not receive a permanent organization until 1801. And even after that little attention was paid to the training of staff officers. Requests for the creation of a formal staff training course, first made by General Radetzky in 1811, went unheeded until 1852. Staff work remained, moreover, in low esteem in the army until finally the debacle of 1866 revealed the consequences of this neglect.

On the other hand, the engineers and the artillery were more fortunate. The ever increasing trend toward fortress and siege warfare, coupled with the considerable rise in the number of guns deployed, made increased technical competence a matter of practical necessity. Then too, unlike the staff, these unglamorous specialists did not intrude upon the jealously guarded personal prerogatives of the commander. Even so, in the realm of the Habsburgs the actual development of technical-scientific training was an uphill fight. In the end, the schools developed not by government initiative and support, but they were promoted and sustained, often at considerable personal sacrifice, by a number of far looking individuals.

In contrast to France there was towards the end of the seventeenth

century a decline in the number of expert military engineers in Austria. Dutch and Saxon engineers had to be employed, both for defensive and offensive operations, in the two great sieges of the period—Vienna in 1683 and Belgrade in 1717. As Prince Eugene complained in 1710, "there is not one among our engineers who can construct a proper fortress or even maintain our existing works." To alleviate this shortcoming, in 1717 Eugene founded an engineering academy, the *Ingenieur Akademie,* in Vienna. But during its first fifty years the academy led a highly precarious existence. After 1718 Eugene's influence declined while Emperor Charles VI was preoccupied with gaining political support for the succession of his daughter Maria Theresa and neglected the military establishment. With a teaching staff of only two, and a state subsidy of less than 1500 florins a year, Johann Jacob Marinoni, its first director and professor of engineering, was forced to search for other sources of income—bequests, inheritances, and tuition fees—to sustain his institution. Even so, the academy, offering a general engineering curriculum, gained a good reputation and among a student body of about 75 in 1730 there were a number of foreigners, including two Americans.

The status of the school did not improve even after 1748, when Maria Theresa began a complete overhaul of her army. Although a Corps of Imperial and Royal Engineers with a total establishment of 98 officers was organized in 1747, the queen was above all interested in her new foundation in Vienna-Neustadt, and when, after almost singlehandedly maintaining the engineer academy for 38 years, Marinoni died in 1755, Maria Theresa was prepared to dissolve the school. At the last minute, however, the school was saved through amalgamation with the "Chaos Foundation," [1] a privately endowed institution for the education of talented noble orphans which in 1752 had been transferred to the state. Amalgamation resulted in the *k.k. Ingenieurschule* in Gumpersdorf near Vienna, which for the next five years functioned under the control of the *Directorii in Publicis et Cameralibus,* the ministry of the interior.

Reverting to military control in 1760 the school reached an enroll-

[1] The curious name, Chaos Foundation, derived from the noble title of its founder, Johann Konrad v. Richthausen (1604–63). As a reward for his services in the mining districts of Northern Hungary, where he "brought order out of chaos," Emperor Ferdinand III in 1653 raised him to the baronetcy with the title "Baron of Chaos." In 1666 the "Chaos Foundation" was established with funds provided in the baron's will for this purpose. In 1715 an additional grant made by Karl v. Moser enabled the foundation to establish an *Ingenieur und Scholarenabteilung,* in fact a privately endowed military-technical school which for several years threatened to overtake the less well-funded *Ingenieur Akademie.*

ment of some 150 students in four classes, with a teaching staff of eleven, drawn in part from serving engineer officers. Nonetheless financial problems persisted and an increasing number of paying students had to be accepted. Room and board ran at 150 florins annually in the 1760s and rose to 1,500 by 1810. The vast majority of students were commissioned in the army, though most of them went into the cavalry and the infantry. Although there had been an expansion of technical troops, these still amounted to only four battalions—sappers, miners, pontooneers, and engineers. For instance during the period 1755 to 1767, 223 graduates took commissions in the "regiments," while only 79 joined the technical services.

After Joseph II, co-regent with his mother after 1765, assumed control over the military establishment the school finally obtained greater, though still inadequate, state support. Curriculum and staff were expanded and after 1784 all students had to pass a probationary year. By 1790 the school, now returned to Vienna, offered an eight year program. While the first four years were preparatory, though with more emphasis on mathematics and geometry than civilian institutions, the second four years were devoted to intensive professional studies, including military administration, though French, rhetoric, horsemanship, and even dancing were not neglected.

By the end of the century then, the Habsburg army possessed a reasonably well-equipped and specialized school to train engineering officers, though financial support remained inadequate and as late as 1851 General Baron Scudier characterized the institution as a "private school under military direction."

Starting thirty years later, the evolution of technical-scientific education in the artillery was more rapid and, due to the purely military application of its subject, free from the civilian elements which characterized the early history of the engineer school. Compared with France, which took the lead in applying science to gunnery, the Habsburg artillery remained backward until the 1740s. The train of artillery was specially raised in times of war and only a small corps of gunners and artificers, still retaining many of its old guild characteristics, was retained in peacetime. The outbreak of the War of the Austrian Succession found Maria Theresa with an outdated and inadequate military establishment. Then in 1744 Josef Wenzel Prince Liechtenstein was appointed Director of Artillery. Although the war was still in progress, he at once established an Artillery Corps School near Budweis in Bohemia.

Given the demands of the war, until 1748 instruction there was confined to practical application, training gunners to deliver more

rapid and accurate fire, but thereafter Liechtenstein expanded the school into a combined training establishment, proving ground, and firing range. The school included military instructors and civilian teachers, a technical library, laboratories, and classrooms where physics, mathematics, chemistry, and ballistics were taught. In the summer months the school moved outdoors for actual practice. By 1779 the school, renamed the *Artillerie Lyceum,* was considered one of the best in Europe and trained a number of outstanding artillerists, including General Gribeauval, the famous reformer of the French artillery service. Money for the program was provided by Liechtenstein, who spent as much as 50,000 florins annually out of his own private fortune.

Moreover, the artillery became the socially most fluid branch of the service. This, of course, was true elsewhere, but Austria was unique because here officers and enlisted men were trained in the same school. Artillerymen looked upon themselves as a class apart. Only the engineers were their equals in specialized training and knowledge. Among the engineers, however, it was only the officers who were trained, while in the artillery many of the rank and file needed specialized skills and knowledge to carry out their duties. The enlisted gunner was a specialist who took charge and fired his own piece; he had to master tables giving angles of ranges and elevations, make allowance for wind and drift, and be able to change the aiming point when necessary. Therefore, at a time when the enlisted ranks were filled from the lowest and most miserable elements of society, the Austrian artillery accepted only volunteers who, moreover, had to have the then still rare educational level of being able to read and write German. Noble connections alone counted for little and enlisted gunners could rise by merit and application. "Whosoever," declared Liechtenstein, "makes a scientific invention of benefit to the service, shall be advanced without regard to person and without any prejudice."

The thrust of these innovations was maintained by Liechtenstein's successor, Franz Ulrich Prince Kinsky, who in 1786 established a unique training unit, the Bombardeur Corps, which provided theoretical as well as practical instruction for officers and enlisted gunners. In 1790 the corps moved to Vienna where many of its instructors held joint appointments at the university. The course of instruction, usually lasting seven years, was most rigorous. The winter semester was devoted to classroom work, the summer to field training and exercises. Subjects taught included arithmetic, higher mathematics, geometry, physics, chemistry, as well as military administration, tactics, surveying, and fortification. Students successfully completing the entire course, usually at age twenty-two to twenty-three, were commissioned into the artillery; those completing only five years became enlisted gun captains and

were posted to the siege artillery. In wartime all studies were suspended and students seconded for service in the field.

Unique in its conception, the Bombardeur Corps provided the Austrian army with efficient artillery specialists and the Austrian artillery generally was considered the best in Europe. Because of its scientific bent, however, it always remained somewhat isolated within the army. Promotion for gunners was notoriously slow and their academic connections suspect. After the revolution of 1848 the Bombardeur Corps was first removed from Vienna and then, in 1851, replaced by the more conventional Artillery Academy in Olmütz.

The last incident illustrates the suspicion of military learning which long prevailed in the Austrian army. To be sure, there were many similarities between the development of technical-scientific military schools in Austria and those in other European countries. But whereas elsewhere, especially in France, these schools were also a response to the new spirit of rationalist, secular, and scientific inquiry, the Austrian rulers did not favor this trend. Therefore, their support was hesitant and they always remained suspicious of the social and moral implications of scientific and technical education, military as well as civilian. Even so, as Albert Sorel once remarked, "Austria always was one behind with ideas and armies, though in the end she always proved to have an idea and an army." Thus, in the eighteenth century Habsburg army, the development of institutions of military learning came about on purely pragmatic grounds.

Sources and Bibliography

There exists very little in English on the Habsburg army during this period. To be sure, there exists some literature on leaders and campaigns, but next to nothing on army administration, training, or military technology. For the period 1660–1683, Thomas M. Barker, *Double Eagle and Crescent* (Albany, N.Y., 1967) provides a good picture of the Habsburg military establishment including military administration and technology. On the general staff there is the article "Command and Staff Problems in the Austrian Army, 1740–1866," in Gordon A. Craig, *War, Politics and Diplomacy: Selected Essays* (New York, 1966). There is nothing in English on the military schools during the eighteenth century.

The standard work on the history of the engineer and later the artillery academy remains Friedrich Gatti, *Geschichte der k.u.k. Technischen Militärakademie* (2 vols.; Vienna, 1901). Much briefer, and also less reliable, is the pamphlet by Moritz Ritter v. Brunner and Hugo Kerchnawe, *225 Jahre Technische Militärakademie 1717 bis 1942* (Vienna, 1942).

For the development of the Austrian artillery there is the old work by Anton Dolleczek, *Geschichte der österreichischen Artillerie* (Vienna, 1887), which can be supplemented by the massive administrative data in Alphons v. Wrede, *Geschichte der k.u.k. Wehrmacht* (5 vols.; Vienna, 1898–1905). Finally, though not all of its essays are of equal value, there is Hugo Kerchnawe, ed., *Ehrenbuch unserer Artillerie* (2 vols.; Vienna, 1935).

Discussion

THE CHAIRMAN (Professor THEODORE ROPP, Duke University): John Shy summed up the discussion, I think, in an extraordinarily interesting fashion. I would suggest that you direct your questions to any member of the panel. Their microphones are open, and if they can't hear the question, I will try to interpret for them.

QUESTION: I would like to ask Professor Bien if he would comment on the influence that the establishment of the twelve *écoles militaires* had during the pre-revolutionary period. I think, for example, of the one that Bonaparte himself attended at Brienne. There has been little reference to these yet. Here the boys, which they were at the time, spent about five years, and they did have mathematical studies.

Professor BIEN: I didn't mention it, and I made an error in not specifying the period about which I was talking. The phase that I described is specifically that of the 1750s, 60s, and early 70s, and a very important change took place after that. Not that mathematics disappeared. The curriculum was questioned by some officers, but there was another question. A number of military people were thoroughly dissatisfied with the program that involved totally insulating these future army officers from society. It appeared that insulation wasn't producing the kind of effect that they had hoped for. In 1776 they decided to disperse students through first ten, then twelve provincial military schools, and to offer more widely a course of study that really looked very revolutionary and attractive by the traditional standards of the classical curriculum. They admitted not just the *élèves du roi*, who were paid for by the king, but also other nobles and even non-nobles. They thought that this was better for the future officer who would now get the right military education and would also see something of society. And on the other hand, there was the hope now also to bring in those people who would be high officers, marshals and generals. The *École militaire* had been training only subaltern officers. Its students didn't have wealth, established position, or court connections that would have permitted them to become colonels, generals, and marshals. By the 1770s some reformers thought that they could begin to train not only the lower officers, but that they might attract to the new education also those who by their wealth were certain to fill the higher ranks. So,

the new arrangement was to do something for the *élèves du roi* by getting them out of the Paris school where the routine and isolation did in fact resemble those of a monastery, and also to attract paying students who would rise quite high.

Professor CLARK G. REYNOLDS, University of Maine: In the discussion of the impact of science and technology on the military from 1700 to 1850, it seems to me there is a preoccupation perhaps of American military historians for land warfare in Western Europe, and I think we shouldn't go away from here without thinking about the naval questions. Because the period 1700–1815 is such a tremendous period of change in naval history, someone should have been represented on one of these panels whether this morning or this afternoon, because it is a whole separate problem and furthermore involves a case study of Great Britain which is unique. Secondly, I am still worried about the great gap of eastern Europe. For instance, the close relationship between the French and the Russian military systems all the way from Peter the Great to the present. Mr. Rothenberg's discussion about the education of enlisted men in this field, due to reforms of the Russian army about the end of this period, was highly significant. So, I would simply like to ask, "Was there any sort of impact on the Russian system by these French reforms of the late 18th century?"

Professor ROTHENBERG: Well, there was a degree of exchange of information between armies in the 18th century. I forgot to mention—since my time was running out—that General Gribeauval, the later reformer of the French artillery service, got his original training in Liechtenstein's artillery school. Simultaneously, the Russians had military attachés—we would call them today—in Austria; and General Peter Shuvaloff, the creator of the Russian artillery service, did try out some of his new pieces in Austria and got back reports on their effectiveness. It seems to me, however, that the interconnection between the services in eastern Europe is not so much based on the French model, but that the Russians learned much from the Austrians and Prussians. I have seen numerous instances of officer exchanges and even whole regiments which go from the Austrian into the Russian service. I have seen relatively little in the Russian service of the French. Now the Turks, on the other hand, took heavily from the French. This is true especially in 1737–39. But I have not come across, except in the Shuvaloff episode, any real, systematic exchange of ideas. I keep on seeing things in the realm of the practical—in 1758 eight experimental howitzers were sent from the St. Petersburg arsenal for further testing in Vienna—things like that. But I have not seen any intellectual ex-

change other than on the impersonal level of manuscripts, treatises, and so on.

Professor SHY: The question was asked about Russia. It seems to me that Russia was militarily backward. That may be my own ignorance speaking more than anything else. But, when focusing on this problem of science, technology, and military development, I think you have to look at those military powers that were—or seemed to be—leading and which set standards against which others measured their own performance and organization. We are not talking about more or less independent societies going their own way. Military history above all is international history, and the principle of equal time doesn't necessarily hold.

Professor REYNOLDS: But the Russians did develop naval infantry and they maintained galley warfare decades beyond the battle of Lepanto, and they used it very well against the Swedes, against the Turks, and so forth.

Professor SHY: But we are talking, aren't we, about science, technology, and warfare—not about everything concerning military history.

THE CHAIRMAN: No. I suggest we have covered an awful lot of ground from the stirrup expert to Moltke in this one day's sessions. Now we have time for one more question. Professor Crowl?

Professor PHILIP A. CROWL, University of Nebraska: In isolating the question of the impact of technology on science and the study of, or the education for, war we overlooked, deliberately perhaps, the impact of technology on other aspects of war. Now could we say that technology in the 18th century had a greater impact on war and on the education for war than it did on agriculture, on industry, on this, that, and the other thing?

THE CHAIRMAN: Well, we're back to whatever one calls that precondition or interstructure and so on, which is what bothered me earlier, and for which, I think, the historian of technology, of which I am not, has no code word as yet. Professor Hughes, what do we call this kind of formative period which is precisely what we are talking about?

Professor HUGHES: In answering the question, I also have in mind some of the comments of Professor Shy. Usually we think of the British Industrial Revolution as dynamic and mechanical and find it of great interest because of these characteristics. Civil engineers, however, tend to focus upon technological developments that can be characterized as static and structural. The technological changes in

France in the 18th century seem of great importance to today's civil engineers—perhaps as interesting as the British Industrial Revolution—because there were major developments in the theory of construction and major building achievements: Military engineers were responsible for many of these. The theories were certainly imperfect by our standards, but we should not discount them too sharply. Even today, engineers tend to be tolerant of approximations. Even approximations, stated as encompassing explanatory generalizations, provide guidelines, give confidence, and stimulate action. Earlier this morning, crude theories of earlier centuries were dismissed too lightly; I would not dismiss the imaginative concepts of the 18th-century French military engineers. To conclude, may I call attention again to the achievements of the engineers, achievements I surveyed in my comment upon Professor Bien's paper.

THE CHAIRMAN: I think all further questions should be directed personally to members of our panel. I thank you for a most enjoyable session.

The Third Session

THE IMPACT OF SCIENCE/TECHNOLOGY ON 20TH CENTURY WARFARE

Introductory Remarks

THE CHAIRMAN (Professor BERNARD BRODIE, UCLA): I have promised not to give a speech, but after all, you do need some introduction to the twentieth century after yesterday's sessions. I might help do so by telling you something that occurred only yesterday at noon. I happened to startle my good friend, Colonel Bill Stewart, by telling him quite casually that my first military experience was in a horse-drawn, field artillery regiment.

One of the reasons I'm here today is that at the age of 16 I happened to be crazy about horses, and since I had no money, the only way I could gratify this peculiar desire was by joining the National Guard, in Chicago, where we rode horses. And by the way, when we were told by our officers, "Horses will always be used to tow the field artillery," I didn't believe them. Horses seemed to me to be terribly vulnerable, especially to aircraft. We were using a kind of harness, three pairs in tandem, which had been used not only in World War I but in the American Civil War; and indeed, the McClellan saddle we were using was named after the Civil War general who introduced it. It had been an atrocity ever since. We practiced on a gun which we considered pretty modern, though it had been designed in 1897. It was the famous French 75, not even an American gun.

That wasn't so terribly long ago. I did add to Colonel Stewart that we should not have been using horses even then. But we were, and those in charge seemed to be content. It was only about 15 years from the time I left that service to the time I ended my second military service, which was in the Navy during World War II; and at the close of World War II, as you all know, there were already nuclear weapons and a ballistic missile. The missile was a German one, the V2. Within 6 1/2 years of that time, I was a member of a small group at RAND, a committee of three, who had the task at the beginning of 1952 of going to Washington to brief the Air Force on the fact that in November of that same year a thermonuclear weapon was going to be tried, and that it would almost certainly be successful.

I am telling about events that covered a span, altogether, of about 22 years. There have been complaints that nothing was said yesterday about the Navy. Well, I shall try to remedy that by pointing out that

when Admiral Nelson was killed at Trafalgar in 1804 aboard the flagship *Victory,* the ship was then 40 years old. Of course it had been rebuilt several times because of rotting timbers, but it was the same ship in design, and it had exactly the same guns that it had carried for 40 years, smoothbore 32-pounders which fired only solid, round shot. Thus, Admiral Nelson could learn his trade and exploit it without fearing that technology would take the ground out from under his feet. Well, I promised you I would not give you a speech, but this much you can forgive me.

[The Chairman then introduced Professor Holley.]

THE EVOLUTION OF OPERATIONS RESEARCH AND ITS IMPACT ON THE MILITARY ESTABLISHMENT; THE AIR FORCE EXPERIENCE

I. B. Holley, Jr.

Duke University

Some twenty-odd years ago I wrote a book called *Ideas and Weapons.* That book put forward the thesis that technological advances may lead to better weapons, but these innovations are exploited effectively only insofar as suitable doctrines are devised to govern their strategic and tactical employment. And from this notion a corollary thesis logically follows: effective doctrine requires a suitable organization with a process or procedure devised to keep that doctrine ever abreast of advances in science and technology. When invited to present a paper on Operations Research, it occurred to me that Operations Research was, in a very real sense, a weapon, a tool at the disposal of command. Why not apply the thesis from *Ideas and Weapons* to this novel tool? [1]

What has been the Air Force experience with Operations Research? Has the Air Force developed suitable doctrines to govern the exploitation of this administrative weapon? What organizations have been contrived to keep this doctrine up to date? In a brief paper one cannot hope to answer these questions fully, but it may be possible to give some indication of what the Air Force experience has, in fact, been and to provide at least a few insights while doing so.

Let me make my position lucidly clear at the outset. I am a historian, not an OR specialist. I make no claim whatever to scientific competence as an OR analyst. But at the same time, it is also true that I am a convert. I believe in Operations Research; my conversion dates back to World War II. At the time I was an instructor in an

[1] I. B. Holley, Jr., *Ideas and Weapons* (New Haven, 1953).

aerial gunnery school. For many months I had been teaching young airmen how to aim their guns to hit attacking aircraft. Our lessons, largely derived from British sources, revolved around the problem of estimating the proper lead, much as one does when duck hunting. We taught what we ourselves had learned, and we taught it in good faith, the best we knew how. But then one day we were told that everything we had been teaching was all wrong.

This was a shocking and humbling experience. How many of those names on the roll of honor in the headquarters building, those gunners killed in action over Germany or in the Pacific, were my fault? Badly shaken, we instructors had to learn all over again. Instead of teaching gunners to lead, we now discovered they must lag; instead of aiming ahead of the fighter, now in seeming defiance of all common sense, we apparently had to aim behind it! This was all most disconcerting.

The new art of "position firing" and the mysteries of the "pursuit curve," we found, were the work of some distinguished scientists who called themselves Operational Research analysts. That was the first time I had ever heard of the phrase, but I was impressed. Even if their instructions seemed to defy common sense, they worked; they got results, and that is what counted. The whole story of how an astronomer from the Mount Wilson Observatory, a zoologist from Washington University at Saint Louis, and a math instructor from Johns Hopkins, among others, worked out the necessary formulas deserves to be told even though it cannot be told here. My only point at the moment is that from that day onward I was persuaded that Operational Research was a new discipline to be taken seriously.

From the time of Archimedes onward, history is replete with examples of military commanders—and industrial managers—who have used a form of Operations Research to improve their effectiveness.[2] But not until the era of World War II did OR acquire its elaborate institutional basis and widespread military application, beginning with an Air Ministry unit established in 1937.[3] From the British, especially the work of P.M.S. Blackett, the line of descent to the US is obvious. Many individuals recognized the exciting potential of OR for the Air Force, but one of the best perceptions was that prepared in 1942 by

[2] For some interesting precursors in the field of OR, see W. F. Whitmore, "Edison and Operations Research," *Operations Research* 1 (1952): 83–85; and H. K. Weiss, "The Fiske Model of Warfare," *Ibid.* 10 (1962): 569–70.

[3] E. C. Williams, "Reflections on Operational Research," *Operations Research* 2 (1954): 441; and Great Britain, Air Ministry, *The Origin and Development of Operational Research in the Royal Air Force* (HMSO, 1963).

W. Barton Leach, a Harvard law professor, and Ward F. Davidson, an industrial scientist of the National Defense Research Council. They acted at the request of the Joint New Weapons Committee of JCS, spurred on by its chairman, Vannevar Bush of MIT. Leach and Davidson pointed out that RAF experience had shown how unit commanders, swamped by the massive influx of data on new weapons and current tactics, could profitably use the help of analytic minds to assist them. Free from pressures of command, thoroughly competent analysts, after lengthy, painstaking, and uninterrupted labors, often using highly sophisticated mathematical or statistical techniques, could extract the significant conclusions otherwise buried in the mass of evidence and quite beyond the reach of a military staff overwhelmed by routine military duties.[4]

Leach and Davidson also laid down certain ground rules. The analysts should be civilians; they must report only to their own unit commander, thus remaining free for the utmost candor. The commander, in turn, would remain free to accept or reject the advice given; analysts would have no command responsibility. To head the activity, it was suggested that the candidate be a non-scientist, a mature lawyer or a trouble-shooting business executive, someone capable of selling the findings of the analysts to the commander in comprehensible terms.

By the end of the war every one of the numbered Air Forces had an Operations Analysis organization of one sort or another, each manned with a mixed bag of physicists, engineers, statisticians, lawyers, economists, astronomers, biologists, or journalists.[5] The results of their efforts were nothing short of spectacular. In the 8th Air Force alone, where bombing accuracy in 1943 was only 15% (15% of bombs dropped fell within 1000 feet of the aiming point), by 1945 this had moved up to 60%. Put another way, the work requiring a thousand bombers in 1942 could be done in 1945 by some 250. Improved efficiencies of this order, repeated many times over in other contexts, won the enthusiastic support of Air Force commanders. As a consequence, Operational Research emerged from the war with a considerable body of opinion strongly favoring its continuation in some form or another.[6]

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[4] W. B. Leach and W. F. Davidson, "Summary Report on Operations Analysis," 1 Sep. 1942, mimeo, Maxwell AFB Historical Div. archive 160.81121-1.

[5] The term "operations analysis" was selected as being somewhat broader in scope than "operations research." Analysts were expected to use OR techniques as but *one* of the methods available to them.

[6] For brief summaries of Air Force OR activities and OA units during the war, see W. B. Leach, "Operations Analysis in World War II," 1948, Maxwell AFB Historical Div. archive 143.504; and LeRoy Brothers, "Operations Analysis in the USAF," *Operations Research* 2 (1954): 1–16. See also "Operations Analysis, Headquarters Army Air Forces, Dec. 1942–July 1947," n.d., Hq USAF, Office of Air Force History.

The problem, then, is before us: could this new tool, this wartime triumph, be successfully incorporated into the permanent structure of the Air Force? Just because it had worked and worked wonderfully well, did it follow that the same kinds of efficiencies could be reduced to routine practice in peace? This was the challenge presented to the Air Force and the informing theme of our present investigation.

The Transition to Peacetime

With the end of the war the OA units were denuded. Virtually everyone was anxious to get out and get home immediately. For the scientists the problem was especially acute. The war in the Pacific ended in September; if one cut out and returned to the campus immediately, one could make the academic year just starting. If one lingered to see what the Air Force might offer, it would be too late for the university and one might have to wait for another whole year. So the Air Force ended up with a tiny caretaker OA unit at Headquarters, largely concerned with demobilizing individuals.

The authorities seemed to agree that some kind of Operational Research capability was desirable, but who would staff it and where should it be located? Since the OA units of the war period were civilian and had no formal Tables of Organization, there was no automatic survival based on pure momentum or Parkinson's law. And with many of the able civilian analysts departed, there were few left to study the matter.

There were plenty of problems: should the permanent organization be civilian or military? If the former, could really able analysts be recruited within the rigidities of Civil Service? Would truly imaginative men be willing to work within the restraints inevitable in a permanent bureaucratic organization? Should the OA effort be in-house or farmed out on contract? Or a mixture of both? Where should the activity be located? Should each major commander have his own service, or should a Headquarters office provide general Operational Research services for all echelons? Where should the Headquarters OA office be located: In the Scientific Advisory Board? In the Research and Development staff, the Training staff, or the Operations staff? It is interesting to note that some effort was made to prepare a history of OR during the war, but no formal publication emerged. Just how far the organizational experience of the war years was analyzed to inform the decision-makers for peacetime remains unclear.[7]

[7] The early postwar efforts to place OA in the Air Force structure can be traced in Maxwell AFB Historical Div. archive 168.64–28.

The outcome of this preliminary skirmishing looked fairly impressive—on paper. A newly drafted Air Force Regulation, 20–7, gave the Director of the Headquarters OA office responsibility for coordinating all Operational Research programs, but there was a marked disparity between this broadly defined obligation and the status of the organization itself. By the end of 1948 as the newly "independent" Air Force began to function normally, OA found itself buried far down within the Headquarters hierarchy, at one time in a subsection of the Directorate of Training and Requirements, at another on a similar level in Operations. Moreover, the OA office was limited to a staff of ten analysts.[8]

In an effort to integrate the analytic effort into normal staff work, the new Director, LeRoy Brothers, developed close ties, at the working level, with the military staff. An excellent and experienced analyst himself, he enjoyed a reputation for turning out reliable studies of the highest technical caliber. His conception of OR is probably implicit in the kind of men he recruited for his own shop, and for the OA units scattered through the commands which he helped to staff. In 1947 the score stood at six physicists, five mathematicians, three statisticians, seven engineers and one maverick recorded as an educationalist.[9] Evidently the non-technical leadership originally recommended by Leach and Davidson had gone by the board along with the idea of using lawyers, economists, political scientists, and others of like leanings.

Despite the limitations under which the OA office labored—its questionable status, few analysts, and uncertainty as to its mission—the organization produced a good deal of solid work in the decade following the war. Increasingly the analysts were called upon to undertake "feasibility checks," calculations of the effort required to obtain a given strategic objective and the likelihood of success within a probable range of vulnerabilities. For example, they computed what the losses would have been in the bomber assault on Germany if the enemy had possessed the proximity fuse. This was the typical pattern of their work: starting with a hypothesis based on the experience of World War II, the analysts would make theoretical extensions based on new assumptions, new weapon systems, tactics, etc., and then predict probable outcomes. Along with these "planning" studies, of

[8] Operations Analysis Summary Report #6, 1 Sep.–31 Dec. 1948, Maxwell AFB Historical Div. 143.504A. For the official definition of mission and function, see AAF Reg 20–7, 11 Oct. 1946, and AFR 20–7, 5 July 1949, which superseded it. Copies in Hq USAF, Office of Air Force History.

[9] Memo, L. A. Brothers for Maj. Gen. E. E. Partridge, Operations Analysis since VJ-Day; A Summary Report, 2 May 1947, USAF GOA file "Organization."

course, there was a steady output of classical OR projects, accident analyses for example.[10]

So the Operations Analysis organization gradually settled down into a routine existence. But one cannot fully appreciate the evolution of the Operations Analysis office by looking at it in a vacuum. It can only be understood in a larger context, in the complex environment in which it struggled—along with every other Air Force organism—in its ecological setting.

THE POLITICAL AND INSTITUTIONAL SETTING: 1946–1958

One important element shaping the destinies of the Air Force OA organization was the Operations Research Society of America, commonly called ORSA. The founding of ORSA in 1952 clearly reflected the increasing appreciation for the work of analysts in both military and industrial circles.[11] From its infancy, the Society favored a "hard science" approach. This is not surprising since statisticians, mathematicians, and physicists made up most of the founding group. There was an effort, however, especially on the part of Ellis A. Johnson of the Army-sponsored Operations Research Office, ORO, affiliated with the Johns Hopkins University, to include substantial representation from the social sciences in the Society's inner councils. But these attempts were largely unsuccessful. Control of ORSA and the editorial policy of its journal remained firmly in the hands of the math-physics types. This is not to imply the existence of a sinister plot on the part of an in-group of hard-science men, but rather a logical outcome of an effort within ORSA to establish Operations Research as a rigorous discipline if not as a branch of science in its own right.

One member of the Society probably reflected the consensus when he put OR into a spectrum that extended from the physical sciences at one extreme, to the arts or social sciences and humanities at the other extreme. He pointed out that in the physical sciences it was customary to investigate by manipulating one or two variables at a time within a strictly controlled environment. At the other end of the spectrum, in the arts, the number of variables was so great that it

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[10] For an accessible account of the work done by the Hq AF OA office, see Brothers, "Operations Analysis in the USAF."

[11] The founding of ORSA is formally described in the *Journal of the Operations Society of America*, vol. 1, as the publication was then described. Also, interview with participant, Dr. George E. Nicholson, Chairman, Dept. of Statistics, Univ. of N. C., 20 Jan. 1969.

was virtually impossible even to formulate a systematic description.[12] Under such circumstances it is little wonder that OR analysts, self-consciously endeavoring to establish their discipline as truly scientific, were hesitant to embrace the social sciences and the arts end of the spectrum where the data inputs were almost always inexact and to a great extent non-quantifiable. While it is difficult to assess the exact degree of influence ORSA and its hard science orientation had on the Air Force OA organization, one may speculate that it was substantial.[13]

Yet another external factor shaping the character of Operations Research in the Air Force was the evolution of other agencies and organizations to perform the OR function for the military services. Both the Army and the Navy built up substantial activities along this line. After virtually ignoring the tool during World War II, the Army established a strong office and within a decade was spending at an annual level of ten million dollars, half of it in-house and half through outside contracts. But insofar as the Air Force was concerned, the most important external agency doing OR work was the RAND Corporation. Established under Air Force auspices as an independent corporate entity in 1948, capitalized by a $100,000 seedcorn Ford Foundation grant, and supported financially by Air Force contracts, RAND by the end of a decade had a staff of 800 and was averaging 400 studies a year.[14]

As a direct offspring of the Air Force high command, RAND seemingly enjoyed a favored status. Its staff reflected a broader spectrum of talent than was to be found in the Operations Analysis office at Air Force Headquarters. Moreover, no Civil Service regulations imposed restraints on RAND recruiting. Over a period of time its effectiveness was further enhanced by the practice of assigning exceptionally able Air Force officers for a year-long "sabbatical" tour at RAND, studying problems in depth. For good reason RAND enjoyed direct access to and the sympathetic support of the uppermost echelons of command.

In general, then, whenever the Air Force had problems for analysis relating to matters of broad national security policy and long-range

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[12] W. C. Randels, "Some Qualities to Be Desired in Operations Research Personnel," *Operations Research* 4 (1956) : 116.

[13] For an example of OA orientation toward a pure science outlook, see LeRoy Brothers, "Education for Operations Research," *Operations Research* 4 (1956) : 415–21, with its suggestion of "sabbatical" leaves for OA analysts to universities.

[14] A brief résumé of military OR appears in A. W. Boldyreff, ed., "A Decade of Military Operations Research in Perspective—a Symposium," (1958), in *Operations Research* 8 (1960) : 798–860. Unfortunately, the paper on the AF OA office given at the symposium was not submitted for publication.

planning, these were normally sent to RAND. In several instances, RAND had already taken the initiative in addressing itself to such issues, particularly those problems having budgetary implications. On the other hand, problems on day-to-day operational matters, generally of a narrower technical nature, were referred to the OA offices.

Rather than inhibiting the evolution of the Air Force OA establishment, RAND in a sense assisted by communicating a widespread appreciation of the need for Operations Research studies within the Air Force.[15] When, for example, a highly sophisticated analysis was sent from RAND to the Air Staff suggesting a line of action contrary to the desire of, say, the Commanding General of the Air Defense Command, the only fully effective reply was to undertake an equally sophisticated analysis of the original RAND study. The Headquarters OA office acquired considerable popularity from its ability to render a high quality of scientific services in the form of *independent* analyses. One must underscore independent; OA analysts thought of themselves as detached professionals with no organizational axe to grind, no weapon system or institutional arrangement to defend. Like the Inspector General, they saw themselves as rendering dispassionate judgments when asked to do so.

Soon, however, virtually every major organization within the Air Force had learned that the best defense against a scheme proposed by the outside professionals, the PhDs at RAND or elsewhere, was to have a PhD or a whole roster of them on the staff or on contract to call up for counter-battery fire when threatened. One amused observer of this process describes it as "the battle of the doctors" as the standard reply to "our PhD says . . ." soon became: "Well, our doc says . . . ." The addition of PhDs may or may not have improved the quality of the staff papers being produced at Air Force Headquarters, but here we are only interested in the impact of this phenomenon upon the OA office.

The analysts of the OA shop at Air Force Headquarters consciously prided themselves on the objectivity and professionalism which characterized the studies they produced. If at times they were tempted to fret at the location of their organization, far below the salt, there was compensatory satisfaction in the realization that the upper echelons recognized and appreciated not only the usefulness but the objectivity of their work. But one question nagged them: this was the proliferation of OR studies being performed by outside contractors

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[15] This and the following paragraphs are based largely upon interviews by the author with participants, supplemented by correspondence.

and others directly for various Air Force agencies. Would they, in their labors, exhibit the same kind of scientific detachment on which the Headquarters OA shop had built its reputation? Wouldn't these hired analysts be under an overwhelming temptation to come up with the kind of answers they thought their hosts would like to get? Even a Commanding General who wishes to hear the hard truth about his own organization sometimes finds it difficult to do so. One is reminded of the comment of the elderly cardinal to his young colleague who had just received the red hat: "You will never again in your life eat a bad meal or hear the truth spoken."

The Headquarters OA shop had other problems, too. Not least among these was the matter of maintaining an effective relationship with the analysts assigned to the various subordinate component commands of the Air Force. To what extent, for example, could the Headquarters Director exercise any meaningful control over the character and quality of the work being done by analysts within the Strategic Air Command? This was the period when airpower doctrine was largely embodied in the concept of massive retaliation. Plans for wartime contingencies concentrated on the delivery of nuclear warheads. In this environment, SAC was pre-eminent. It regularly received the lion's share of men, materiel, and money from the resources available to the Air Force. Thus SAC, from its earliest beginnings, was able to build a strong staff of analysts, some 15 in 1948 at a time when the Air Force Headquarters shop had only ten. And while some semblance of infrequent contact existed between the two groups, the SAC analysts charted a more or less independent course.

The dilemmas confronting the Director of OA at Headquarters are manifest. If he wished to supervise or even to replicate the work done by analysts out in the Commands, he was largely forestalled by the simple fact that analysts in the field reported to their own commanders and not to him. The experience of World War II had shown that without this arrangement, analysts in the field would never get the local cooperation essential to their success. So there was little expectation of any change in these command relationships. Whatever supervision Headquarters OA was to exercise over the field would be by mutual desire—not by directive. At the same time the scope of the Headquarters OA organization seemed at least in some measure threatened by the penchant to use outside contractors, particularly on long-range policy studies. This practice may in some respects have been in response to the implicit desires of the Headquarters analysts themselves. For they were in no small degree committed to a purist approach in their studies by their desire to maintain an impeccably

scientific standard of performance so as to be always acceptable in the sight of the newly emerging profession symbolized by the Operations Research Society of America.

The veteran Director of OA finally gave up the on-going struggle to exploit Operations Research in the Air Force to its fullest potential. After more than a decade of pioneering effort in which much had been accomplished, he returned to the university world where, incidentally, he distinguished himself. This was in 1958. By that time the former Commanding General of SAC had become Vice Chief of Staff and the top leadership was fully committed to the view that SAC, the embodiment of all that had been learned about heavy bombardment in World War II, would for some time to come continue to represent the best weapon system for massive retaliation in a nuclear environment. In this context it seemed natural, almost routine as it were, when a former analyst and scientific advisor to the Commanding General of SAC was appointed as the new Director of Operations Analysis at Air Force Headquarters.

THE OPERATIONS ANALYSIS OFFICE IN ACTION: 1959–1967

The advent of a SAC man as Director of OA at Headquarters brought a rather dramatic quantum jump in the status of the organization; it moved up to become an independent office reporting directly to the Vice Chief of Staff, which is to say, reporting directly to the former Commander-in-Chief of SAC. The new Director was a vigorous and forceful individual who soon transformed the character as well as the focus of the OA office. In place of a somewhat passive service agency, providing Operational Research studies on request, he built, or rather attempted to build, a more aggressive organization that would take initiatives toward becoming the catalytic agent for OR throughout the whole Air Staff. In some ways the Director brought to his position a fresh perspective; he looked outside of OA as well as within. He sensed a need to focus the analytic resources available to the Air Staff and questioned the logic of having these resources fragmented under diverse leadership.[16] In some circles these thoughts had appeal, in others they were written off as the dream of an empire builder. Whatever the merits of these views, there were no dramatic mergers of any analytic groups. To some degree this result should have been predictable. Within the service departments—short of the secretarial level—there has always been resistance to placing too many

[16] For an illustrative example, see undated memo (approx. June 1960), COA to Generals Strother, Estes, and LeMay, USAF GOA "Organization" file.

critical resources under direct civilian management. And the Director of OA, it must be remembered, was a civilian, not an officer in uniform. But the proposed mergers, whether inspired by long-range vision or naked ambition, were not without merit; and to some extent they were prophetic of the McNamara era and things to come. The Director, however, did not continue to push the merger idea. In June 1962 he abruptly left government service to accept a position with industry.[17] This unexpected move triggered a widespread search for a new Director. But not until February 1963, did the Secretary and the Chief of Staff announce that they had again turned to the Strategic Air Command to fill this key position. The man they appointed, Paul Hower, had impressive credentials in the field of military operations research and was at the same time thoroughly acceptable to the scientific community.

One of the most urgent commitments made by the new appointee was a promise to upgrade the OA staff. Of course, the promise was much easier than its fulfillment. The Civil Service System, with all its merits, is not conducive to weeding out marginal employees with any degree of alacrity. Progress in this direction was agonizingly slow. Moreover, it proved difficult to attract to government service highly qualified replacements who met the rigorous standards demanded for analysts. To offset this circumstance and also because of the value of incorporating military experience within the analyst staff, the Director prudently obtained authorization for the addition of 12 military analysts to his office. That any such number of qualified analysts with advanced degrees were to be found in blue suits is a telling index of the appreciation for Operations Research which had begun to permeate the Air Force by 1963.

The widespread appreciation for the *potential* of Operations Research as a tool of command was, of course, a distinct advantage. Moreover, even after the departure of the former Director, with his close personal ties to the Vice Chief of Staff, the OA office retained its upper echelon status with direct access to the top command. But this elevation had its disadvantages, too. The top-level leaders had been looking increasingly to OA for analytical studies on critical problems of command. And unfortunately for those analysts who enjoyed performing the kind of rigorously scientific investigations for which they had been trained, a subtle but nonetheless profound change had taken place in the kinds of questions being asked.

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[17] By the time of his departure, his SAC patron had moved up to Chief of Staff and access was less easy. Moreover, the Chief's term had less than a year to run, and it was not then certain that his tour would be extended.

The emphasis now was on a new kind of study called Systems Analysis rather than on Operations Research in the classic mold. Systems Analysis was something broader than OR; it was, to oversimplify, an extension of the OR techniques developed during and after World War II to problems of a larger context and longer range, problems of force composition, questions relating to the allocation of resources within the Department of Defense, and the selection of weapons for development from many competing alternatives. These alternatives had to be examined with regard to costs as well as differences in performance. Where the analysts of the war years had worked to make the performance of *existing* weapons optimal, the task now was to make *future* systems more effective. So the emphasis shifted to studies of alternative strategies, to the determination of requirements posed by these strategies, and to the promulgation of the detailed performance specifications demanded of each weapon system by the strategy selected. Assistant Secretary of Defense Charles Hitch defined Systems Analysis as "a continuous cycle of defining military objectives, designing alternate systems to achieve these objectives, evaluating these alternatives in terms of cost effectiveness, questioning the objectives and other assumptions underlying the analysis, opening new alternatives, and establishing new military objectives." [18]

The Korean war and the successful detonation of a nuclear device by the Russians followed by US intelligence intercepts of Russian successes in launching intercontinental ballistic missiles had helped to raise the defense budget four-fold to a new peacetime plateau in the neighborhood of 40 billion dollars a year. In this threatening climate the recommendations of John von Neumann's "Teapot Committee" in 1954 had touched off an enormous national effort on the order of a Manhattan Project to perfect missiles capable of delivering nuclear weapons to any point on the globe. The outcome was a rapid proliferation of radically different delivery systems and a remarkable compression of the traditional sequence of design, development, prototype testing, and production. Formerly it had been possible to spend a decade in trial and error; if there had been doubt as to the relative merits of the B–29 and the B–32, both were developed and carried to production. That was no longer possible; now the menace of The Bomb and the inordinate costs of development suggested that trial and error had become trial and

[18] Charles Hitch, "An Appreciation of Systems Analysis," *Operations Research* 3 (1955) : 466–81, and "Plans, Programs and Budgets in the Department of Defense," *Ibid.* 11 (1963) : 1–17, quoted at p.8. The most prolific expounder of systems analysis has been Alain C. Enthoven. For one example, see "Systems Analysis and Decision Making," *Military Review* 43 (1963): 7–17.

catastrophe—fiscal as well as nuclear. No wonder those in command wanted Systems Analysis to help them.

As Assistant Secretary of Defense Charles Hitch observed, even in World War II where it was possible to weigh the relative merits of two bombers with, say, ten variables, there were still more than 1,000 combinations to consider. When the number of alternative weapon systems was raised to no more than four, the number of possible combinations requiring evaluation ran up to more than a million.[19] So Systems Analysis became the vogue-word, the in-thing at the Pentagon. And in the light of the mission of the OA office as defined by regulations, it would certainly appear that analysts on the OA staff would have much to contribute in providing critical inputs to the decision-makers.[20] But appearances are deceiving. There were a number of practical difficulties that lay in the way of turning OR analysts into Systems Analysis men.

To begin with, as Hitch and others have indicated, the whole business of Systems Analysis was still in the embryonic stage. The methodologies available to those who would practice it were less than satisfactory. It was, as he bluntly put it, more art than science. The big difficulty lay in the uncertainties. How could one predict with any accuracy the length of time it would take to get a weapon system operational or what it would cost in five or ten years? Confronted with such unknowns, analysts shifted their tactics. Instead of looking for "the best" solution, they now had to be content with "a better" solution, sometimes settling for nothing more than an indication of where the critical sensitivities lay with no hope whatever of exactitude. Hard data inputs, the quantification of which is the OR specialist's metier, were increasingly difficult to get. As one analyst put it, the "data diet" had become dangerously inadequate. With full-scale maneuvers prohibitively costly or physically impossible in some instances, and banned by international treaty in others, realistic tactical experience grew thinner and thinner and more reliance had to be placed on extrapolations from ever more remote combat actions of the past.[21]

Analysts trained in the mathematical tradition of OR, proud of their scientific identification, had good reason to hesitate before entering the arena of Systems Analysis with all its intractable uncertainties. One

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[19] Hitch, "Appreciation of Systems Analysis."

[20] That the new Director expected to move boldly into Systems Analysis on taking over the OA division is clearly indicated in his memo describing his expected role, AFGOA to AFCVC, 20 Mar. 1963, USAF GOA "Organization" file.

[21] Charles Hitch, "Uncertainties in Operations Research," *Operations Research* 8 (1960): 437–45; D. W. Meals, "Trends in Military Operations Research," *Ibid.* 9 (1961): 252–57; and Palmer Osborn, "Selecting Weapons Systems," *Ibid.* 9 (1961): 265–71.

keen OR man in industry saw the problem clearly. Operations Research is at the crossroads, he warned. Unless we take care, OR will become narrowly identified with mathematical techniques alone. And if this happens, OR analysts will find themselves entirely "excluded from the deliberative and decision-making circles." But not all OR men wanted to run with the decision-makers. Some suggested that perhaps the high command had been oversold on the potential of OR as a technique.[22] Perhaps the leaders in the upper echelons were asking OR specialists to tackle problems they could not hope to solve. Perhaps it would be better to stick to problems that lent themselves to rigorous analysis and scientific solutions. After all, as the great methodologist E. Bright Wilson once said, "Many scientists owe their greatness not to their skill in solving problems, but to their wisdom in choosing them." [23] Might not the wise course for dedicated analysts be one of self-denial, sticking to one's professional last while eschewing the headier upper atmosphere of Systems Analysis?

The debate over whether or not OR men should plunge into Systems Analysis was not confined to the ranks of the Air Force analysts. The question was discussed vigorously in the Operations Research Society of America.[24] The phenomenal growth in the membership of this society affords us something of an index to the rate at which OR had come to be accepted as an important discipline in the United States. In scarcely more than a decade after its founding the society had nearly 5,000 members. Parenthetically one might observe that it took the American Historical Association some 60 years to achieve a comparable growth. The size and stature of ORSA is significant because it suggests the existence of strong professional pressures on analysts to maintain the scientific standards of their calling by avoiding the baffling and intractable problems at the soft end of the data spectrum.

One suspects that fear of losing professional respect was not the only restraint keeping OR men from moving vigorously into Systems Analysis. As one perceptive analyst expressed it, when the scientist leaves the realm where knowledge is king, he must compete with other skills and adopt another life style. When the scientist enters the world of command, he finds the decision-maker dominated by problems thrust upon him, not those which happen to interest him. Where the scientist's allegiance is to truth, the decision-maker's allegiance is to the organization he

[22] Meals, "Trends in Military Operations Research," and Osborn, "Selecting Weapons Systems."

[23] E. Bright Wilson, *An Introduction to Scientific Research,* p. 1, quoted by Hitch, "Uncertainties in Operations Research."

[24] See, for example, Hugh J. Miser, "Operations Research in Perspective," *Operations Research* 11 (1963): 669–77.

serves. The decision-maker says, "What must we do now?" not "What can we learn here?" If the scientist expects to sit in the top councils and enjoy the pay and prestige of such positions, can he hope to retain the immunities and academic detachment normally associated with a scientific role? [25]

Little wonder, then, that the analysts at Air Force Headquarters showed a certain reluctance to push into Systems Analysis. By limiting themselves to more or less classical OR projects at the operational level, traditional optimization studies built on solid data, they could make a contribution and still retain their professional standing as scientists. In practice, then, the OA shop settled for a more modest role compatible with its historical beginnings and with the environment in which most of its analysts had traditionally functioned.

Was it really possible for OA to stay out of Systems Analysis and still remain useful as a Headquarters staff agency? During the McNamara era the name of the game was cost effectiveness, and even an organization of modest ambitions, quite apart from any desire to avoid the soft or non-scientific end of the data spectrum, could scarcely avoid becoming involved. Indeed, so urgent was the need for Systems Analysis studies within the Air Force to cope with the insistent demands coming down from the Department of Defense, that several alternative types of in-house analysis organizations were considered by the Air Staff—in some respects, not unlike the merger or consolidation that had been suggested and discarded several years previously. The Director of Operations Analysis, Paul Hower, was invited to consider becoming the Technical Director of a large, new organization with a staff combining virtually all the military and civilian analysts whose work impacted directly on all planning and programming activities at the Air Force Headquarters level. Whether this suggestion reflected a meaningful appreciation of OA on the part of the high command or simply a gesture of personal respect toward its leader, is a matter of speculation. The offer was tempting, but the Director of the Operations Analysis shop resisted the temptation. Why? What were his motives? Certainly he was concerned for the preservation of professional standards. A meticulous craftsman himself, was he reluctant to see his organization diluted and the technical character of its work drastically altered? Did he foresee that a move into Systems Analysis would require a staff of economists and social scientists rather than the hard science, math-physics types he had on board? Or did he foresee that this type of consolidation, however superficially appealing, was an over-reaction to the problem of fragmented analytic

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[25] J. B. Lathrop, "Operations Research Looks to Science," *Operations Research* 7 (1959) : 423–29.

resources? Whatever his motives, in the best scholarly tradition, he decided to oppose consolidation. He was successful in resisting the change.

Of course, this still left the need for an organization to do the long-range, force composition studies requiring large staffs of military officers and some supporting analysts. To fill this void a Studies and Analysis office was formed with a clearly defined role that was complementary to rather than competitive with the OA office.

But what of OA? Did the OA staff, having backed away, at least temporarily, from the big job in Systems Analysis, initiate an aggressive program of Operational Research looking to the field commands where so much of the real world action was to be found? A certain amount of analytic work relating to current operations was going on at the Pacific Air Command (PACAF) and at several other centers. In South Vietnam, where it was most needed, however, the response, at least initially, was poor and the product mediocre. This left the Headquarters OA shop in a vulnerable position. Both the new Chief of Staff and the Vice Chief were enthusiastic supporters of Operations Research, and they expected a high quality performance from the organization. So too did Dr. Harold Brown, the Secretary of the Air Force.

Secretary Brown was unusually well equipped to understand the potential of OR as a tool of defense management. His years of experience at the Livermore Radiation Laboratory and as Director of Research and Engineering in the Department of Defense not only gave him a general familiarity with the techniques of OR but, in addition, a demonstrated capacity to apply the tool himself. Convinced that the existing OA organization was not being exploited to the utmost by the Air Force, Secretary Brown and the Chief of Staff initiated an independent review of the OA organization.

The sudden death in 1967 of the highly respected Director of OA, Paul Hower, the man who had backed away from Systems Analysis, coincided with the review triggered by Secretary Brown. In view of the significant dissatisfaction with the analytic studies, and the lack thereof, flowing back from Vietnam, some observers anticipated that the OA organization was in for rough sailing ahead. Fortunately, the outside investigators called in to appraise the situation moved with dispatch, and so too did the Air Force high command.

In the first place, there was no delay in appointing a new Director. An unusually well-rounded man with wide experience both in and outside the Air Force was chosen to fill the slot. The new appointee, Ross Thackeray, not only had strong academic credentials in math and

physics, but also had studied social psychology as a Rhodes Scholar. He was obviously the kind of man who could deal comfortably with the whole data spectrum and not just the hard science end. Moreover, as a newcomer with no need to be defensive about the past performance of his organization, he was in an excellent position to elicit the best that was within the Operations Analysis shop. And this he set out to do with energy and imagination as he responded to the findings of the outside consultants who had been probing his organization.

These outside experts who had been called in to appraise the Air Force experience with Operations Analysis were highly critical of much that had been done and even more critical of what had *not* been done. But they also recognized that the key to the difficulty was organizational. Although the existing regulation granted the Director of the Headquarters OA office responsibility for "the general direction" of the Operations Research program throughout the Air Force, in practice he had no real authority to fulfill this obligation. The OA structure in the Air Force consisted of 16 separate offices, each reporting to its own local commander. The so-called OA organization was in fact a loose confederation of virtually sovereign states. Although the Director in Washington did have authority by regulation to assist in the recruitment of staff for all OA units, he had no control whatever over the programs and products generated outside his own Headquarters office.

Armed with the findings and recommendations of the outside consultants, the new Director was able to secure a redrafting of the basic Air Force regulation governing Operations Analysis. The revised regulation strengthened the Director's hand considerably.[26] Henceforth the promotion of all senior analysts within the worldwide OA organization would be accomplished by a panel at Headquarters USAF. Moreover, the Director was given clear-cut responsibility, and some semblance of authority, for the coordination and dissemination of all analytic studies produced by the OA units anywhere in the Air Force. Finally, under Secretarial mandate, the Director established an entirely new OA unit at Headquarters 7th Air Force in South Vietnam with an authorized staff of six civilian and five military analysts.[27]

The new team created for the 7th Air Force in Vietnam during 1968 was the first successful initiative taken by the OA organization to field a full-time effort in the combat theater. But 1968 was rather late in the game, for the fighting had been going on for many months by then. It was almost as if Operations Analysis had become so institutionalized, so

[26] AFR 27-7, 5 Apr. 1968, superseding that of 5 Oct. 1959. See also Change 1, 12 Aug. 1958.

[27] AFR 27-7, 5 Apr. 1968, section 5a (4).

tied up in its own internal bureaucratic organizational problems, that it had forgotten its *raison d'être*, its original purpose and practice, spelled out so effectively in the splendid improvisation of World War II. True, there had been analysts working at various theater headquarters, but there was little contact between these men and the tactical units at the bases from which the missions were being flown.

While the new, permanent OA organization with 7th Air Force was being developed, the Director in Washington dispatched several ad hoc groups to the combat theater. They were to address themselves to one or another of the more pressing problems confronting the combat forces there and return promptly with quick fix solutions. One example will have to suffice. At the instigation of the Deputy Secretary of Defense, OA was asked to evaluate the effectiveness of B–52 operations in Southeast Asia with an eye to determining the most appropriate sortie rates. A team of analysts, headed personally by the Director, went out on short notice to Vietnam.

Once in the theater the analysts found themselves confronted with an incredibly complex problem.[28] The B–52 sortie offers a unique form of massive firepower in support of ground operations. But the fluidity of ground operations and the resulting lack of precision in pre-strike intelligence makes the task of evaluating alternative targets extremely difficult. Even where post-strike assessments are possible, damage to tactical targets, such as a Viet Cong regiment on the march, is difficult to measure. Unlike the factories and bridges and other similar targets of World War II, the targets of today are troop concentrations, rice stores, hidden jungle encampments, and moving columns of cargo cyclists. Damage to such objectives is difficult to quantify by exact measurement. How do you quantify the impact of a sortie which scatters and immobilizes a Viet Cong battalion during a crucial phase of friendly ground operations, even if a subsequent body count reveals few if any casualties?

The analysts found, however, that the B–52, for all its design as a strategic bomber, was proving highly popular as a tactical weapon. If testimonials from ground troops could be taken as evidence, the B–52 was a success. To the Army, the B–52s offered a "free" multi-division reserve available to friendly forces under attack. They also provided an interdiction force playing a role not unlike that of traditional cavalry, defending exposed flanks and thus permitting ground troops to make otherwise hazardously deep penetrations into enemy territory, to mention but two of many such uses. In sum, the ground troops welcomed the B–52 that made it possible to take objectives with greatly reduced loss of

[28] Based on interviews with Arc Light team members 24 Oct. and 12 Dec. 1968.

life. But opinions such as these, while gratifying, were no substitute for statistical measures or hard evidence on cost effectiveness.

The various ingenious means by which the analysts compared the costs and capabilities of the B–52 with the performance of conventional artillery and the performance of fighter-bombers need not occupy us here. Suffice it to say that even in the span of several days their studies opened up a whole series of problems for investigation, each offering opportunities for improving the effectiveness of air operations by a large factor. War is inherently wasteful. Improvements in yield by a factor of five, ten, or even twenty are still just as possible as they were in World War II. What is required is the imaginative and resourceful application of highly qualified brainpower by men with broad experience and analytical minds, men with wit enough to know there is more than one way to skin the cat.

For example, since impact areas were often inaccessible and hard to quantify in conventional terms for bomb damage assessment, the analysts adroitly turned the problem upside down. Instead of evaluating damage, they switched to an intensive study of how targets were selected. If, say, 100 targets are nominated each day by ground force commanders, intelligence units, and others, and only five strikes per day can be mounted, by what process are the candidates for bombing selected? Does this process actually select the most lucrative targets? What priorities prevail? How objective is the selection? What steps can be taken toward optimization?

Although the results of the B–52 study may never be published in their entirety, it is possible to say that the analysts have given the Air Force some hard data from which to make a case for the B–52 as a modern airborne analogue for cavalry and artillery. Far more important than the findings of this particular report, however, is the new focus and attitude of the OA analysts at Air Force Headquarters. The organization is in the business of Operations Research in a highly positive way. A new vitality animates the staff.

Only time will tell whether or not Operations Analysis has at last reached the Promised Land. Meanwhile, what can we learn from these twenty-odd years of wandering in the Wilderness? Historians are not interested in praise or blame, only in understanding. In this frame of reference a few observations may be ventured.

No one will deny that Operations Research as a tool of command has not always been used to its full potential. It is also a fact that the spectacular success of a makeshift, temporary, ad hoc collection of inspired amateurs during World War II has not always been matched by

the new professional organization of the postwar period. Perhaps this was impossible. But even if one makes allowance for the fact that the powerful psychological thrusts of war are not present in peace, the evidence still suggests that OA fell short. It is probably true that a bureaucratic organization recruited under the hampering constraints of Civil Service cannot hope to compete with the free-wheeling latitudes of wartime. It is only fair to note that despite such restraints, OA still managed to recruit some excellent analysts. It must also be said that some of the most penetrating criticisms and some of the most valuable insights encountered in the course of the research for this paper came from interviews with one or another of those old-line Civil Servants who would self-deprecatingly call themselves "entrenched bureaucrats."

Like most other such organizations, OA has its bell-curve of talent from good to bad. But the occasional failure to maximize the effectiveness of OA was, one suspects, as much a matter of organization and top management, of structure and leadership, of the decision-maker's conception of the role of Operations Analysis as it was a matter of shortcoming or lack of proficiency on the part of the individual analyst.

If one fact stands out above all others, it is the absence of historical analysis, self-conscious, introspective, analytical concern for the ongoing OA organization and its processes. Organizational studies were few and far between; only one or two of those yet uncovered could claim any significant depth of analysis.

In sum, then, when confronted with a new tool, Operations Research, the Air Force has been slow to develop an adequate doctrine to optimize its use. What is more, the Air Force was also slow to develop an adequate process to keep OR doctrines abreast of the changing times. The skills of the statistician, the mathematician, and the physicist are doubtlessly essential to Operational Research. But one may be permitted to speculate on whether or not the Operations Analysis office of the Air Force might not have derived some benefit from a significant infusion of political scientists, economists, historians, or, for that matter, even the lawyer Barton Leach and Ward Davidson had suggested way back in 1942.[29] But then, had this taken place, OA would most certainly have developed into a rather different discipline than it has in fact become.

This paper is a preliminary survey; it makes no claim to definitive interpretation. It was built on a limited amount of research. Some facts of significance have almost certainly been omitted from the reckoning.

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[29] For two perceptive British commentaries groping for doctrine, see R. W. Bevan (Air Ministry Scientific Advisor's Dept.), "Trends in Operational Research," *Operations Research* 6 (1958): 441; and E. C. Williams (Admiralty, Dept. of Operational Research), "Reflections on Operational Research," *Ibid.* 2 (1954): 441.

The main objective here has been to trace the broad outlines and to suggest a thesis which will, one may hope, point the way for further and deeper investigation. The author will be pleased if this effort provokes contradictions and amplifications in the interest of fuller understanding.

Let me conclude by suggesting that what has been said here of Operations Research may well be true of other management tools and weapons for decision-making in the Air Force—and for that matter in the Department of Defense as well as industry. Surely the evidence here presented suggests that even in an organization as overwhelmingly technical as the Air Force, there is still a large place for political scientists, historians, and others similarly trained to ask searching, probing, difficult, and often embarrassing questions of their technologically oriented colleagues.[30]

[30] I am especially indebted to W. Barton Leach and LeRoy Brothers for their letters shedding light on OR in the USAF.

Commentary

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Professor Holley's account of the 25 year tribulation of operations research in the Air Force fairly allots operations research to the special class of "good" things (like the American Revolution and Queen Victoria) designated in that paradigm of military history, *1066 and All That*. He assumes that operations research has a high intrinsic value. And although the assumption is probably correct, it is advisable to recall R. D. Schmerl's classic observation that "to make knowledge an end in itself, . . . is very close to doing things for the hell of it." [1]

Which should suggest that I do not in all respects agree with Professor Holley's generally cheerful conclusions or with the reasoning that produced them. Or, indeed, with his way of describing his reasoning.

I take exception to Professor Holley's analysis and his findings on various grounds. First, it would appear that he has been entirely too charitable to the Air Staff and too forgiving to the practitioners of operations research at Air Force headquarters. He has not stated plainly some adverse conclusions and he has not set down some harsh judgments that—to me, at least—are implicit in his resumé. Second, but perhaps not entirely independent of the first, his careful account of how the Air Force has reacted to the reality of operations research mentions few names except those of the dead or the long retired—and not all of those. It is not difficult for a reasonably diligent reader-between-the-lines to discover that people named Zimmerman and LeMay were among the anonymous principals, although intuition will not tell anyone what was so controversial about their respective roles. Individuals and their actions are the corpus of history; numbers and abstractions have become the province of mathematicians and the like. We owe it to ourselves as

*The views expressed in this paper are those of the author. They should not be interpreted as reflecting the views of The RAND Corporation or the official opinion or policy of any of its governmental or private research sponsors. This paper has been published as RAND report P-4114.

[1] Rudolf B. Schmerl, "The Scientist as Seer," in *A Stress Analysis of a Strapless Evening Gown and Other Essays for a Scientific Age*, ed. Robert A. Baker (Englewood Cliffs, N. J., 1963), p. 186.

historians, I maintain, to spit in the occasional eye that wants spitting in. Otherwise, we might as well become political scientists.

Next, I do not agree with Professor Holley or the Operations Research Society of America that there is a special brand of analytical thought which occurs at the knee of some peculiar curve and becomes purer than something else called Operations Analysis, or Systems Analysis, or even—if you will pardon the phrase—Cost-Benefit Analysis. It does not make a great deal of difference whether one gets his Monte Carlo distribution by throwing dice or by reading between the beeps of an IBM Model 360. The numbers don't care. And since the point of it all is to recommend solutions to specific individual problems, there would appear to be some native advantage to assigning dimensional values to as many relevant uncertainties as can be identified in each problem. I know Professor Holley essentially agrees with that doctrine, even if he does not say so here, because he has explicitly used it in one of the finest studies of Air Force—or Air Forces—decision-making yet written, his *Buying Aircraft: Matériel Procurement for the Army Air Forces,* in the Army historical series.[2]

Finally, to paraphrase Oscar Wilde, it does not matter much whether an analysis is conducted in the Pentagon or on the third level below Offutt Air Force Base or in Santa Monica, if it is done well. That is all that matters. It is plain from Professor Holley's account, although he has been extremely careful to avoid unfounded criticism, that Headquarters, United States Air Force, was spectacularly unskillful in exploiting the potential of operations research, but it is also apparent that one must exercise extreme care that the implications of such findings do not unfairly prejudice evaluation of the analysis operations of other agencies, institutions, or headquarters which have not been explicitly examined.

The precepts of operations research are not new. Liddell Hart observed in October 1937 that "the way that decisions are reached on questions of strategy, tactics, organization, etc., is lamentably unscientific." He urged that the investigation of problems "be given to a body of officers who can devote their whole time to exploring the data on record, collecting it from outside, and working out the conclusions in a free atmosphere."[3] Liddell Hart had more to say and much that was equally pertinent, but that is the crux of what may be the first and certainly is one of the best statements of a requirement for operations research.

[2] Irving Brinton Holley, Jr., *Buying Aircraft: Matériel Procurement for the Army Air Forces* (Washington: Department of the Army, 1964).

[3] Basil H. Liddell Hart, *Thoughts on War* (London, 1944), p. 125.

As the British saw it, operations research had two initial and two subsequent aspects. First there was the evaluation of the *operational* performance of an equipment or a weapon, and second an analysis of the relationship between tactics and weaponry to see to what extent one influenced the form of the other. Two extensions of operations research appeared later. One concerned the prediction of the course of future operations which might be either tactical or strategic, with the object of influencing policy. The last had to do with the study of the efficiency of organizations in actual operations.[4]

It seems evident from a comparison of (a) the British notion of what matters were within the purview of operations research with (b) the actual experience of the postwar USAF in these matters, that the British view was very much the broader. Headquarters, United States Air Force, seems to have kept its beak pretty much on the first line of inquiry opened by the British, and although the Tactical and Strategic Air Commands may have tried excursions into the relationship between tactics and weaponry, they were field commands and dared not venture into issues affecting changes in strategy, or organizational evaluation. Was there some peculiar element in the British experience that led them to such a generous view and something else in the American experience, or the American establishment, that caused quite a different perspective to result? These are legitimate questions, but it is not likely that they can be fully and satisfactorily explored here.

Still, something can be gained by a quick look.

Before the start of the European War the R.A.F. had three fundamental experiences of operational research. The first involved the influence of radar, newly developed, on air tactics. The second was an attempt during the special bombing trials of 1937–1938 to discover the accuracy of bomber attacks on various targets and the effect of antiaircraft fire on low altitude and dive bombing attacks. The third involved experiments with methods of controlling the interception of intruding bombers and ultimately led to the creation of control room or operations room procedures.

Significantly, in all cases the principal inquiries were conducted by civilians who were mostly specialists in the engineering sciences, and the results were in all cases contrary to the hopes and beliefs of principal military figures and many senior civilians. There has been some

[4] This explanation of the span of operational research is taken from a speech made in 1952 by E. C. Williams, Director of Operational Research at the Admiralty, and cited in *The Origins and Development of Operational Research in the Royal Air Force* (London: HMSO, 1963), p. xviii.

discussion here of the difficulties that occurred when traditional military authorities of earlier centuries were obliged to face unpalatable technical realities. An observation of Sir Solly Zuckerman bears on this point, in part because he says in four sentences what others have taken forty pages to recite:

> The soldier must have faith in his weapons. Someone, somehow, must make 'the man at the sharp end' believe that the weapons with which he has been provided are at least as good as those that the enemy or potential enemy has at his disposal. . . . This world of faith and belief, of service, loyalty and discipline, is the very antithesis of the one in which science thrives.

He added:

> Perhaps . . . it is to the professionalism and isolation of the military establishment . . . that we have to look more than anywhere else in order to understand the fact that until quite recent times the military mind has been suspicious of the changes which are provoked by technological advance—and correspondingly suspicious of scientists.[5]

It is reasonable to suggest that here and in the reaction of operations research specialists to the expression of such suspicions lies one source of the shortcomings of postwar operations research as practiced in Air Force headquarters. Operations research became part of an established organization: it should be quite obvious even to the most insensitive observer that no organization ever succeeds in reforming itself. Yet in matters of strategy analysis and evaluation of organizational effectiveness, as well as the probable relationship between weapons and tactics, a part of the organization—the operations research function—was nominally charged with forcing the head of the organization to consider actions he would instinctively reject.

Prudent men do not take such positions, and prudence seems to have been characteristic of most postwar operations analysis in the USAF. Operations research tended to confine its attention to matters that were highly quantifiable and to avoid the doctrinal controversies implied in the British definition of the function. Whether that was the consequence of organizational placement, as Professor Holley suggests, or of the preferences of those who guided the function, or of the sociological setting of operations research in the military society that surrounded it cannot be readily determined. But these, too, are legitimate questions that must ultimately be answered.

Some years ago, Charles Poore commented that ". . . a great measure of the historian's trade lies in expertly pointing out what was inexpertly

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[5] Solly Zuckerman, *Scientists and War; the Impact of Science on Military and Civil Affairs* (London, 1966), pp. 8–9, 13.

done long ago. Or not done."[6] Professor Holley has explicitly denied any such intent, but nonetheless he has indirectly and somewhat too gently told us what the Air Force has not done or has done quite inexpertly during twenty-odd years of tinkering with operations research in Air Force headquarters, both as a function and as an institution.

Operations research as it was conceived and practiced during World War II represented a means for performing more effectively or more efficiently tasks that the military services would somehow ultimately be obliged to perform in any case. In those earlier and more violent days, "effective" and "economical" implied lesser casualties and slighter wastage of materiel than would otherwise occur. As tends to be true of all military establishments, everywhere, and at all times, the Air Force, having discovered that operations research was a particularly useful technique for specific applications, decided to enfold it in the existing structure of a permanent organization. But the Air Force seems to have been blind to the reality that both the circumstances that made operations research initially valuable and the characteristics of the discipline were perishable.

It is an interesting commentary on the character of operations research as used by the United States Air Force and as remarked by Professor Holley that its first significant contribution was to improve the bombing effectiveness of B–17 and B–24 aircraft in 1944, and its most recent accomplishment to recommend ways of improving the bombing effectiveness of B–52 aircraft over Vietnam. It would seem that in 25 years the designations of the aircraft and the targets have changed, but not much else.

Operations research began by addressing quite small issues—or at least issues that could be addressed in rather small terms. Bombing accuracy, gunnery practices, maintenance concepts, supply and inventory problems: these were the wartime topics. And although such topics remained important to the postwar Air Force, they were overtaken and subordinated to much larger issues of weapons choice, strategic doctrine, procedures of research and development, methods of ensuring inter-service cooperation in combat conditions, and such matters. Operations research in the Air Force generally has not sought out such larger questions or, in approaching them, has attempted to narrow the uncertainties by excluding consideration of items that are difficult to quantify. Here is a sub-aspect of the problem: the difficulty of handling large policy issues in an organization designed for smaller questions. Moreover, Professor Holley observes, the operations research organization in Air Force headquarters preferred to deal with matters that lent

[6] *New York Times* Book Section, 29 Dec. 1964.

themselves to quantification rather than those in which judgment factors had to be substituted. Over the long term the operations research function seems to have avoided any broad commitment to do broad-issue analysis.

Another reality of the interaction between technology and its military applications is cost—the economic factor. There may have been a time in the postwar world when questions of military choice could be decided without weighing cost consequences, but they probably were very small issues. Certainly there has never been a goal "worth any price." That is a preposterous exaggeration of need. But it has become very difficult to induce the services—including the Air Force—to make hard choices that require giving up one desirable objective in order to finance another. Perhaps the internal structure of a military society cannot endure the continued shock of making choices that are quite unacceptable to some part of the society—as in deciding to invest in missiles rather than bombers, for example, or armed helicopters rather than close support fighters. In any case, the service that would be obliged to live with the consequences has habitually been reluctant to make broad value judgments in matters that affect choices between weapons and—hence—force structures. Force structure decisions hinge on prior choices of strategies. Or should, although in fact strategy choices are definitely limited by present force structure realities—the very high probability that Soviet assured destruction forces cannot be destroyed, for instance—and by force structure expectations that frequently are dominated by technological uncertainties. But these are precisely the sorts of matters that operations research practitioners in Air Force headquarters were least anxious, and perhaps least ready, to consider. For reasons that are beyond the province of this and Professor Holley's paper, the services (all three, not the Air Force alone) tried to avoid making unpleasant force structure recommendations, preferring to let others have the responsibility, and the onus. One consequence has been an increasing intrusion of secretariat-level authorities in questions that once were decided by operating commands. Such intrusions have occurred, and have subsequently been institutionalized, either because a service refused to make choices, or because a service made such irrational choices that senior authorities concluded that they could no longer trust service judgments.

Here are issues and questions to which the techniques of operations research might properly have been applied. At least they are not foreign to the interests of the function. But instead separate systems analysis organizations were created during the late 1950s and the 1960s at the secretariat and air staff or command level. In some respects that may have been a minor tragedy of organization. But it may also have been

inevitable, given the nature of organizations and the character of the assignments of systems analysis organizations. In any event, the trend definitely cut away one branch of operations research.

The plain facts seem to say that the postwar Air Force appreciated the worth of operations research and the advantages of keeping it alive. So the function was institutionalized and the capability preserved against some future emergency. Not much more seems to have been contemplated, and owing to (a) the preferences of the operations research specialists and (b) the pressures of ordinary bureaucracy, scarcely that much resulted. Operations research did not take on the varied tasks suggested by early experience, but the tasks had to be done and ultimately other organizations tried to do them.

Professor Holley has gently discussed one of the reasons that such "other organizations" within the Air Force were also only modestly successful in performing such difficult assignments of analysis. The systems analysis organizations were in many instances used as resources to generate evidence that could be used to counter the findings of analysis performed by groups outside the Air Force. Put more baldly, they frequently served as protectors of the status quo, or of the preferred status, whether quo or not. They were no more able than any other part of the larger organization to bring on major changes, however necessary. The usual source of such change in a thoroughly institutionalized organization like the Air Force was (a) an investigative body, (b) a consulting organization, or (c) an executive committee.

Whether such realities were acknowledged or not is in some respects immaterial. The creation and growth of RAND and of later organizations which purported to do about the same kinds of research was one consequence of the failure of organizations native to the Air Force to bring about essential changes.

RAND was called into being to consider the weaponry implications of new technology, a set of questions operations researchers of the late 1940s were extremely reluctant to attack. In the succeeding five years, RAND ventured cautiously into a consideration of the doctrinal and force structure implications of new weaponry. After 1953, it was unlikely that any internal group of 10 or 20 Air Force headquarters people who called themselves operations researchers would be able to recapture and cope with such complex, difficult, and fascinating problems.

Nor were the immediate intra-organization alternatives accepted. Cost-effectiveness analysis and scenario analysis were treated merely as the new faces of operations research, and Air Force headquarters does not appear to have agreed that these new faces belonged at the military conference table with all of the older faces. There lies another difficulty.

Sir Solly Zuckerman put it this way:

> Basically, operational analysis implies a kind of scientific natural history. It is a search for exact information as a foundation for extrapolation and prediction. It is not so much a science in the sense of a corpus of exact knowledge, as it is the attempted application of rigorous methods of scientific method and action to new and apparently unique situations. The less exact the information available for analysis, the less it is founded on experience, the more imprecise are its conclusions, however sophisticated and glamorous the mathematics with which the analysis is done.[7]

As Professor Holley has pointed out, after 1948 operations research in Air Force headquarters rather completely became the province of numerologists. The humanities and the sciences of synthesis were mostly excluded from the discipline as practiced in the Air Force. But not because mathematicians do not like historians and such. Heed for a moment a comment by one of the leading advocates of the systems analysis approach. "Like operations research," said Alain Enthoven, ". . . [systems] analysis can and must be honest, in the sense that the quantitative factors are selected without bias, that the calculations are accurate, that alternatives are not arbitrarily suppressed, and the like. But it cannot be 'objective' in the sense of being independent of values. Value judgments are an integral part of the analysis: and it is the role of the analyst to bring to light for the policymaker exactly how and where value judgments enter so that the latter can make his own value judgments in the light of as much relevant information as possible."[8]

Here is a critical distinction. The heads of Air Force operations research in the Pentagon seem to have recognized intuitively the futility of raising issues the Air Force did not want to face. It is sometimes easier to avoid dabbling in certain classes of problems than to face the consequences of solving them. To suggest that the general ineffectiveness of the Air Force operations research organization can be explained largely by the absence of historians, economists, sociologists and the like is to oversimplify a very complex case. No doubt such people would be nice to have about. One is reminded of Alice's conversation with the white knight about mouse traps and bee hives and anklets on his horse, and the knight's remark that the mice kept the bees away or the bees kept the mice away, but that in any case the mouse trap was a necessary precaution against the possibility that mice would take over the horse's back and the anklets were necessary "to guard against the bites of sharks." It seems extremely unlikely that historians or economists could

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[7] Zuckerman, *Scientists and War*, p. 18.

[8] Alain Enthoven, "Operations Research and the Design of the Defense Program," *Proceedings of the Third International Conference on Operational Research* (Paris, 1964), p. 53.

have helped much to answer questions that were never asked, and it is quite evident that operations research, for one reason or another, did not permit itself to become involved in large complex problems of policy that might conceivably have required the services of non-numerologists.

Once a function has been as carefully defined by its practitioners as was postwar operations research, the function tends to become the nucleus of an institution, and institutions are the stuff of which bureaucracies are made.

One of the dominant attributes of any ordinary bureaucracy like the Royal Navy, the German Post Office system, the Politburo, or the Roman Curia is that it accepts a stable set of values early in its existence and rarely, if ever, changes them of its own volition. Bureaucracies are self-perpetuating. They do not die of neglect—as witness the continued vitality of the United States Indian Bureau—and are decidedly difficult to kill: the Suez Canal Commission still lives, somewhere. Institutions change mostly in their response to outside pressures. If the pressure can be relieved elsewhere, as in the creation of alternative ways of doing essential systems analysis, an institutionalized operations research function will change little and the parent service—here the Air Force—will suffer thereby.

Consider a recurrent question that has perturbed the Air Force for two decades: What kinds of weapons should be selected for development emphasis. As early as 1945 the Air Force, still part of the Army, saw the need of developing and deploying bombardment missiles. Yet it was not until 1957—twelve years later—that the Air Force gave up persistent efforts to develop aerodynamic cruise missiles in preference to ballistic missiles for the bombardment mission, notwithstanding that for several years the greater value of ballistic missiles had been established to the satisfaction of virtually all independent analysts. This question is further discussed in the Appendix to these remarks.

A friend of mine who is far better equipped than I to comment on the development of operations research in the Air Force, or on a paper about its development, has observed with considerable astuteness that the really striking achievements of operations research in the Royal Air Force, where it had its first and greatest successes, occurred while England was losing the War, and at a time when radical notions and outspoken criticisms were listened to because radical measures were desperately needed.

Institutional change is rarely popular and institutional change is particularly unpopular if neither the institution nor its masters can find reason for dissatisfaction with matters as they have been. Let me

close then, by repeating once more Charles Poore's injunction, ". . . a great measure of the historian's trade lies in expertly pointing out what was inexpertly done long ago. Or not done."

Let us begin.

Appendix*

By 1952 or 1953, quantitative analysis had indicated that cruise missiles would be less accurate, less dependable, and more costly (in terms of combat effectiveness) than ballistic missiles. But virtually all of the research leading to such conclusions was conducted outside the regular Air Force, either by independent study groups or by committees created at the insistence of senior civilian officials. The Atlas ballistic missile program is perhaps the best known example of projects so affected. Although proposed as early as 1946, Atlas was continually subordinated to cruise missiles, at first because of assumed technological inadequacies, later because of technological misjudgments intermingled with shortcomings of doctrine. In each instance decisions were reflected in allocations of funds, or nonallocations.

The assumptions of technological inadequacy which hampered missile development from 1946 to 1953 arose in a set of value judgments accepted uncritically by Air Force analysts. The basic assumption was that ordinary evolution from a base of aircraft technology would lead most directly to an operationally capable missile. But there were important underlying assumptions. For example: (1) the assumption that some guidance system that was an extension of autopilot and autonavigator experience would be "easier" to develop than a closed-loop inertial trajectory system; (2) the assumption that derived or evolutionary advances in airframe technology would permit long-endurance, high-speed cruise missiles to be perfected before problems of high-stress launch and high-temperature re-entry could be solved for ballistic missiles; (3) the assumption that high-efficiency turbojet or ramjet propulsion systems would emerge from development much sooner than dependable large rockets; and (4) the assumption that the chief doctrinal modification required to move from bombers to missiles could be satisfied by substituting missiles for manned bombers in about a one for one ratio.

In time it became evident that each of these premises was thoroughly erroneous. They stemmed from assumptions about the value of experience in developing and operating the aircraft of World War II.

*The Appendix was not presented at the Symposium.

From them were derived conclusions about the advisability—and risk—of depending on the evolution of missiles from aircraft progenitors, rather than investing in a ballistic missile program itself.

There were other considerations, too, of course. Until at least 1951 the Air Force was inherently incapable of accepting the commitment of any substantial part of its development-production budget to such exotic weapons as intercontinental ballistic missiles. The establishment of a separate Air Research and Development Command in 1951 removed that particular obstacle. Technology, or its uncertainty, remained an obstacle until 1952, after which time those who looked closely enough into the matter could find evidence that an intercontinental missile was no longer a particularly high risk investment in unlikely technology. In retrospect, it is quite plain that the difficulties of developing a ballistic missile were somewhat less appalling than the unacknowledged difficulties of developing a comparably accurate, reliable, and effective cruise missile. Put baldly, Atlas was much easier and cheaper to develop than Navajo would have been, or Snark, the evolutionary cruise missiles Atlas competed with. One is sorely tempted at this point to apply directly Professor Elting Morison's principal thesis about the resistance of a military society to major change. To people who had grown up with manned bombers before and during World War II and who had mostly stayed with them through the early part of the next decade, a cruise missile was a less painful and certainly a less abrupt departure from what they were familiar with than would be a totally alien ballistic missile. Those who favored the evolutionary approach to the creation of a new generation of weapons, predominantly missiles, were people to whom aircraft had a meaning as a way of life, a symbol, a preferred means of performing a military assignment. With minor exceptions, those who sought to bring on major or revolutionary change had no such commitments, being primarily engineers and scientists of one sort or another, and only secondarily airplane commanders. It is not really important whether the opponents of change consciously recognized the possibility that the appearance of a ballistic missile might lead to the decline and ultimately to the disappearance of the manned bomber. It is enough that those concerned sometimes acted as if they foresaw that possibility. So cultural resistance to the innovation presented by the ballistic missile was one reason for the relatively slow initial progress of that development, and failure to take appropriate account of the unpredictability of technology was another.[9]

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[9] These matters are discussed by E. E. Morison, *Men, Machines and Modern Times* (Cambridge, Mass., 1966), pp. 37-39; and by Robert Perry, "The Ballistic Missile Decisions," RAND Corporation Report P-3686, Oct. 1967.

If the ballistic missile had, by 1952, become technically and financially and culturally conceivable, why was the requirement for it not strongly validated? In retrospect the answer seems plain enough: cultural resistance, or the extreme reluctance of a bureaucracy to change itself. If the analysis techniques developed through operations research and the experience of World War II ever had a promising utility, it should have been in a situation of this sort. What was required was an objective review of established but not widely understood facts and an analysis of the importance and relevance of those facts. That the Air Force had a doctrinal commitment to aerodynamic missiles derived from manned bombers was totally irrelevant to the issues which were clamoring for consideration.

SCIENCE-TECHNOLOGY AND WARFARE; ACTION, REACTION, AND INTERACTION IN THE POST-WORLD WAR II ERA

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Recent newspaper headlines prove that the title of this paper represents an historical fact, not an untested hypothesis. Strident opposition to the interrelationships of science, technology, and warfare testify to the existence and importance of their connection. Students demonstrate against the links of their universities with IDA (Institute for Defense Analyses); faculty at M.I.T. organize a nationwide "science strike" against the involvement of scientists in military research; and Congressional debates on the Safeguard antiballistic missile system bring forth dire warnings against the military-industrial complex.

Yet the close interaction among the scientific yogis, the technological titans, and the military commissars continues unabated, and the trends of the past two decades continue. For the fact is that much of our scientific and technological progress since World War II has been stimulated by military needs,[1] that our defense establishment "has staked its present and future on the application of science and technology,"[2] and science, technology, and warfare are more closely intertwined than in the past. Indeed, it is the thesis of this paper that the interaction between science-technology and warfare is quantitatively greater in the post-World War II era than ever before in history and qualitatively different. Our task is to probe the reasons for this, analyze the facts involved in the interaction, contrast these with preceding periods in history, and compare them with other social parameters.

[1] Jerome B. Wiesner, *Where Science and Politics Meet* (New York, 1965), p. 68.

[2] Ralph Sanders and Fred R. Brown, eds., *Science and Technology: Vital National Assets* (Washington, D. C., 1966), p. 1.

Quantitative Relationships between Science-Technology and Warfare

Two major historical developments testify to the growing quantitative relationships between science-technology and warfare. First is the wedding of the two, which was consummated during World War II. Second is the military revolution of our times which is concomitant with our contemporary scientific-technological revolution—drawing upon it, sustaining it, and stimulating it. Let us look at each of these developments in turn.

The Alliance of Science and the Military

The marriage of science-technology to warfare can only loosely be called a shotgun wedding, for by the time it took place the shotgun was technologically obsolete; it was the atomic device in the bomb bay rather than daddy's shotgun behind the door which brought the marriage about. And, like most marriages, after a brief honeymoon, there have been periods of dullness punctuated by family quarrels over the usual things: the family budget and the schooling and careers of the offspring.

We have become so accustomed during the past three decades to the alliance between science and the military that it is sometimes difficult for us to realize that it has not always existed. Actually, there was a close connection between the two during the first half century or so after the United States came into existence, but then the two drifted apart, with sometimes outright animosity between them until World War II. Let us briefly trace their relationships from the founding of our Republic until the present.

Thomas Jefferson set the pattern for military contributions to science and engineering during the first stage of our country's growth. To Jefferson, the citizen and the soldier were the same. Under the impetus of Jeffersonian ideas American military tradition emphasized, up to the Civil War, those technical elements which were common to both the soldier's trade and civilian occupations.[3] Sylvanus Thayer institutionalized this "technicism" of the West Point curriculum by patterning it along the model of the *École polytechnique*, the great French scientific and engineering school.

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[3] "The good military officer was an expert in a technical skill such as civil engineering, ship design, cartography, or hydrography." Samuel P. Huntington. *The Soldier and the State* (Cambridge, Mass., 1964), p. 193.

During the first 60 years or so of its existence, the United States Military Academy was not so much a professional military academy as a technological and scientific institute serving the entire nation. Engineering, both military and civil, dominated studies at West Point, and its graduates were utilized by the government in topographical surveys, railroad building, and the construction of other internal improvements, while they were also much in demand by private employers for their engineering skills. Indeed, "West Point produced more railroad presidents than generals . . . and the Academy was justified to the country in terms of its contribution to science, exploration, and internal development." [4] When the Naval Academy was established in 1845, it followed the West Point pattern, merely substituting marine technology for civil engineering.

The fact is that the infant United States scarcely needed much in the way of military professionalism but was in dire need of the technological and scientific contributions which the military forces could make. As Clarence Lasby has pointed out, "The national interest centered . . . on immediate and practical problems; it was in two areas of challenge—westward expansion and overseas exploration—that science and the military were to form their initial partnership." [5]

After the Civil War, however, both Annapolis and West Point tended in the direction of military professionalism and shifted away from "technicism." These early relationships between science and the military, despite their fruitfulness, had been based more upon expediency than upon any natural affinity recognized by both sides. The tenuousness of their relationship was demonstrated during the Civil War, when scientists, believing that they were ignored by the War Department, persuaded Congress to establish the National Academy of Sciences to assist in the conduct of the war.[6]

Foundation of the National Academy of Sciences did not mark a new era of collaboration. For the first 50 years of its history, the advice of the National Academy of Sciences was sought by the War Department on only 5 matters: "the question of tests for the purity of

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[4] *Ibid.*, pp. 198–99.

[5] Clarence G. Lasby, "Science and the Military," in *Science and Society in the United States*, ed. David D. Van Tassel and Michael G. Hall (Homewood, Ill., 1966), p. 252. See also A. Hunter Dupree, *Science in the Federal Government* (Cambridge, Mass., 1957).

[6] Frederick W. True, *A History of the First Half Century of the National Academy of Sciences, 1863–1913* (Washington, D. C., 1913), pp. 7–15, claims that American scientists desired prestige and recognition similar to that which their European counterparts received from their governments, and that this was the major impetus to the founding of the National Academy of Sciences.

whiskey; the preservation of paint on Army knapsacks; the galvanic action from association of zinc and iron; the exploration of the Yellowstone; and meteorological science and its applications." [7] The Academy seemed as little interested in assisting military endeavors as did the military in seeking out its assistance. Indeed, the inability of the National Academy of Sciences adequately to respond to military needs led to the creation of the National Research Council in World War I. But this new arm for providing the military establishment with scientific and technological assistance made only a "meager" impression on America's military effort.[8]

The inter-war period witnessed the almost complete severance of even those tenuous ties between scientists and the military which had developed during World War I. Although the National Research Council continued after the war, it took as its mission the application of science to the public welfare, and to most scientists, imbued with the pacifism of the times, this did not include military work. On its part the military, ignoring the lessons of World War I, prepared for the next war as if it were to be a re-run of the Franco-Prussian War. At a time when barbed wire and machine guns had made horses technologically obsolete, the cavalry still remained the "glamour" arm of the service. Even if the military had wanted to foster scientific and technological innovation, a nation intent on pursuing an isolationist position in world affairs and caught in the throes of an economic depression continually reduced military appropriations. The military, still glacierlike in appreciating the possibilities of science and technology, spent its funds on maintaining the equipment in hand, most of it left over from World War I, rather than in developing new weaponry. The result was that the United States entered World War II armed primarily with the land weapons of World War I.

On the eve of America's entrance into World War II, the situation changed rapidly. America's great industrial capacity was quickly switched from civilian to military production, giving us overwhelming quantitative superiority over our enemy in armament and equipment. Civilian scientists, concerned about the nation's scientific preparedness for war, again offered their services to the nation as they had during the Civil War and World War I. The day following the fall of Paris (June 15, 1940) President Franklin D. Roosevelt created the National Defense Research Committee at the instigation of Vannevar Bush and Karl T. Compton. But even before then, in 1939, Albert Einstein at the urging of a group of colleagues, many of whom were refugees from

[7] Lasby, "Science and the Military," 259–60.
[8] *Ibid.*, p. 261.

Nazism, wrote his famous letter to President Roosevelt which marked the genesis of the atomic bomb war effort.

Although the atomic bomb was the most dramatic of the scientific-technological achievements of World War II, it came near the end of the conflict, when the issue had been largely decided; already before then science and technology had done much to bring victory to the allies. Among the major accomplishments of wartime scientific-technological efforts were the development of radar, the proximity fuse, jet propulsion, rockets, bombsights, penicillin, insecticides and rodenticides, the packaging of blood and blood substitutes, and a variety of land and sea vehicles for special purposes.[9]

Cooperation between the military and scientific establishments went remarkably smoothly during World War II. One reason was that the scientific community shared wholly in the aim of rooting out Nazism and Fascism. Scientists were willing to forego their personal researches for the immediate task of military victory.

On the other hand, the military itself was changing its approach to the art of warfare and to the role of science-technology therein. The military career of the late President Eisenhower, as expressed in Hanson Baldwin's eulogistic appraisal of him, illustrates this transition:

> General Eisenhower spanned an age when the big battalions had been dethroned by the big factories as the arbiters of battle. And he was to live to see the "big bang" replace industrial output as the primary factor of a nation's military strength.
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> The development of nuclear weapons, with their awful power to devastate great areas, turned the military clock back during President Eisenhower's lifetime, not to superior numbers, not to superior mobilization potential, but to instantly ready professional forces capable of manning the ramparts of the sky. These were forces far different indeed, from the traditional cavalry and infantry of General Eisenhower's youth.
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> General Eisenhower, therefore, was born into the age of technological revolution in war—an age when general management, rather than personal generalship, and an ability to capitalize on new technical developments were the hallmarks of military success.[10]

During World War II the question of military domination of science scarcely arose. After all, the entire purpose of the Office of Scientific Research and Development (OSRD) was to provide the military with the scientific support it needed.[11] The story of the atomic bomb illustrates the productive division of labor between military men

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[9] James Phinney Baxter, *Scientists against Time* (Boston, 1946).

[10] Hanson W. Baldwin, "The Man as General: a Military Appraisal," *New York Times*, 30 March 1969.

and scientists-engineers. The actual job of producing the bomb was left to the military, with Vannevar Bush and his civilian associates acting as overseers. While this division of responsibility might seem simple in theory, it was more complex in practice, for this type of project had no precedent in the annals of warfare or in the history of technology. General Groves started out with the responsibility for the engineering, construction, and operation of the plants to produce bomb materials; he also took over other responsibilities such as security and counter-intelligence, and ultimately he became responsible for selecting the target cities, preparing orders and instructions for the actual bombing operations, and arranging for supporting army and navy units.[12] On their side, the scientists also found themselves with wider responsibilities than they had at first foreseen, conducting fundamental research in nuclear physics, radiation chemistry, metallurgy, biology, and on clinical problems dealing with pathology and hematology.

Later security issues have magnified the dissension between the military, General Groves in particular, and the scientists, personified by J. Robert Oppenheimer. Yet during the war itself these were but minor irritations which were inevitable given the nature and scope of the task, the limited time available, and other wartime pressures. There is no evidence that these unduly hampered the effective completion of the task.[13]

Hiroshima brought a new and overwhelming factor into the relationship of technology and science to national security. The new magnitude of firepower forced a change in the thinking of military men and it transformed traditional power politics. God, it seemed clear, was no longer on the side of "the biggest battalions," nor would victory go to the one who got there "fustest with the mostest" if the "mostest" consisted of the old cavalry and infantry. Even questions of superiority in materiel, location, or military leadership were subordinated. What mattered now in national security was superior scientific and technological capability. The military, which had long resisted technological changes which might threaten social changes within the

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[11] According to Jack Raymond, *Power at the Pentagon* (New York, 1964), p. 97, Vannevar Bush said that "military domination was a statement that meant nothing whatever under those circumstances." However, in *Modern Arms and Free Men* (New York, 1949), p. 47, Dr. Bush foresaw the possibility of future problems arising from large-scale government support of scientific research.

[12] Leslie R. Groves, *Now It Can Be Told* (New York, 1962), Foreword.

[13] See Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, 1939/1946* (University Park, Penn., 1962), chaps. 3–9.

military establishment itself,[14] now rushed to embrace science and technology.

Within two decades after the close of World War II, General Maxwell D. Taylor, chairman of the Joint Chiefs of Staff, signalized the alliance between the military and the scientists and engineers in his commencement address at West Point on June 5, 1963. Drawing his theme from Ralph Waldo Emerson's famous Phi Beta Kappa speech of 1877, "The American Scholar," wherein Emerson proclaimed the emancipation of American scholarship from dependency upon Europe, Taylor claimed that American military thought had become increasingly independent of Europe ever since the Civil War. Since World War II, according to Taylor, the American military had developed its own doctrines of warfare, devised its own strategy, and produced its own weapons. Most importantly, he pointed out that the close association of the American military with scientists and engineers had brought about the greatest change in weapons since the invention of gunpowder—"the introduction of nuclear arms and their missile delivery systems." [15]

The strength of the military's commitment to science and technology in the post-World War II era can be measured in terms of rising expenditures for research and development. In 1945 only half a billion dollars was spent for military R&D. By the 1968–69 fiscal year, $8 billion of a total federal expenditure of $16.4 billion for R&D, was for defense. While the bulk of this money goes into development, testing, and engineering, rather than into fundamental research, the military has also supported basic research. Indeed, before the National Science Foundation was established, the Office of Naval Research played a major role in subsidizing basic scientific research in this country, and the defense establishment has not wholly relinquished that role.

Apart from DOD allocations, Cold War competition has affected support of scientific research. Once the Cold War had begun, American or Soviet scientists had only to argue that "the other side" was about to overtake them in any particular field of science in order to receive vastly enlarged support. Thus, in the middle 1950s, when the Russians began to build the world's most powerful accelerator at Dubna, the United States Congress quickly acted to provide an even more effective—and expensive—accelerator at Argonne National Laboratories.[16]

[14] An excellent study of this type of military resistance to technological change is provided by Elting E. Morison, *Men, Machines, and Modern Times* (Cambridge, Mass., 1966), chap. 2. See also Thomas DeGregori and Oriol Pi-Sunyer, "Technology, Traditionalism, and Military Establishments," *Technology and Culture* 7 (Summer 1966): 402–07.

[15] Quoted in Raymond, *Power at the Pentagon*, pp. 329–30.

[16] Daniel S. Greenberg, *The Politics of Pure Science* (New York, 1967), chap. 10.

Although research in particle physics, unlike nuclear physics, seems to have no immediate—or even long-range—practical applications, in the minds of Congress—and probably also of the Moscow Presidium—competition in basic science is part of a national military competition. After all, Einstein's theories seemed to hold forth small possibility of military application when first propounded. It would be a rash historian of science and technology, indeed, who would venture to predict that no possible practical application could eventuate from any given piece of basic scientific research.

Another example of the military's infatuation with science can be seen in the number of scientists directly employed by the military. A good example of this is the Air Force Systems Command. Of the 9,800 officers in this Command, about 5,000 are scientists/engineers; of the 33,200 civilians employed by the AFSC, some 6,000 are scientists/engineers.[17]

Indeed, the military have become so enamored of science that they forgive even its shortcomings. Until Secretary Robert McNamara introduced cost-effectiveness into the Department of Defense, the military had the costly tendency to invest in virtually every idea in missilery and nuclear physics that might produce a wonder weapon. Billions of dollars were spent on projects that later were abandoned. Examples include the Navaho guided missile, which cost $679.8 million before it was dropped in 1957; the Snark missile, perfected and declared operational even after it was considered obsolete, was cancelled in 1962 after costing $607.4 million; and the nuclear powered airplane was finally dropped after a nearly 15-year effort costing about $1 billion.[18] Nevertheless, the Department of Defense was later, in its Project Hindsight, to prove that even the expenditures on these misguided missile systems had been fruitful and productive, on the basis that components in the subsystems were later incorporated into successful weapon systems. In other words, even the failure of scientific-technological efforts did not deter the military establishment from its love affair with science. Even when the military establishment, in the person of the naval hierarchy, resisted the introduction of the nuclear submarine, in a manner strangely resembling the resistance to the continuous-aim firing innovation, as described so eloquently by Elting Morrison,[19] the nuclear submarine was ultimately built. This was largely due to the abrasive persistence of one man, Admiral Hyman Rickover, and his

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[17] John B. Hudson, "Management's Critical Challenge—People," *Air University Review 20* (Jan.-Feb. 1969): 61.

[18] Raymond, *Power at the Pentagon,* p. 305.

[19] Morison, *Men, Machines, and Modern Times,* chap. 2.

support by a Congress which yielded to no one its appreciation of the accomplishments of science and technology.

The Scientific-Technological Revolution of Our Time and the Military-Technological Revolution

We have already pointed out that American society during the past quarter century has been involved in a scientific-technological revolution. One proof of the military's interaction with science and technology can be seen in the way in which the concomitant military-technological revolution has fed upon and has fed into that scientific and technological revolution.

We can perhaps summarize briefly the scientific-technological revolution of our time by listing some of its characteristics: a revolution in power sources, deriving from the exploitation of nuclear energy; a materials science revolution, involving lighter elements and new alloys, and new chemical materials which have no counterpart in nature; a transportation revolution, signalized by jet aircraft and spacecraft development; a communications revolution, manifested in television satellites and the computer; a transformation of machine production methods through automated techniques and electronic control systems; an agricultural revolution made possible through industrial organization and the application of science to crops, fertilization, and machinery. In addition to devices and processes, there has been a revolution in the supporting elements of technology: managerial organization, the institutionalization of research and development, the introduction of systems analysis, and recognition of the social science component of scientific and technological innovation. The result has been an acceleration of scientific and technological development, a growing interdependency and complexity of both science-technology and society, and the creation of a larger number of options for human and social choice.[20] Without exception, warfare has shared in all these revolutionary changes.

The examples are so obvious that I scarcely need elaborate on them. Let me cite only a couple. We can, for instance, point to the ways in which the development of atomic-powered submarines and Polaris missile systems has transformed both the strategic missions and operational capabilities of sea warfare. Despite the important role played by submarines in both World Wars, the fact is that the military use of the sea up to some 15 years ago was by systems which were basically constrained by the ocean's surface. Even ships classified as

[20] See Melvin Kranzberg and Carroll W. Pursell, Jr., eds., *Technology in Western Civilization* (New York, 1967), vol. 2, especially chaps. 2 and 3 by Peter F. Drucker.

submarines were, in fact, surface craft which were submersible for short periods of opportunity. In 1954 the USS *Nautilus* became operational, and for the first time a true submarine system could come into being. The critical item was, of course, the nuclear reactor, but the nuclear submarine also exhibited another characteristic of modern technology, namely, the interdependency of various sciences and technologies, for many other advances were necessary before a true submarine capability could be obtained. These included: (a) life-support systems capable of sustaining a large crew in a small and nearly closed ecology; (b) precise navigation without dependence on surface aids; (c) communication systems; (d) hull structure and hydrodynamic form to permit significant excursions from the surface; (e) sensors and sonars for extension of the range of underwater visibility; (f) underwater egress for launching large missiles and weapons; and (g) computer-aided, integrated ship and weapon controls. Added to this was the ballistic missile. "This total set of new capabilities, when applied to all forms of submarine warfare, are so totally different from those of the World Wars that they can hardly be regarded as the same system." [21]

Another example is the development of airborne digital systems for navigation. During World War II, ground-based aids to navigation—radio and radar beacons and air-to-ground radar mapping—had been developed; although these provided very accurate fixing of location, they were subjected to interference and could not always give the coverage needed. The problem was to develop self-sufficient systems by improving the discrimination of air-to-ground radar, coupled with an inertial system and a computer. Now we have airborne doppler radar to measure ground speed, automatic astro-tracking, inertial platforms, and electronic computers. Navigation is becoming more and more automated, thereby increasing the accuracy of weapons and eliminating possible sources of crew error.[22] Similarly, the development of satellites for military observation and perhaps as weapons platforms promises to do the same for airborne warfare as for submarine warfare, namely, development of a virtually self-sufficient military capability without reliance upon the surface of the globe.

And, of course, within each of these air and sea systems there are minor technological revolutions: miniaturization, computers, laser beams, power controls, television, etc.[23]

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[21] John P. Craven, "Ocean Technology and Submarine Warfare," in *The Implications of Military Technology in the 1970s*, Institute for Strategic Studies (London), *Adelphi Papers* No. 46 (March 1968): 38–40.

[22] Christopher Hartley, "The Future of Manned Aircraft," *ibid.*, pp. 32–33.

[23] See also Edward Bennett, ed., *Military Information Systems: the Design of Computer-Aided Systems for Command* (New York, 1964).

Warfare has also shared in the managerial aspects of the scientific-technological revolution. Indeed, the military establishment itself has provided the most dramatic illustration of changes in the decision-making process. This was largely the doing of Secretary of Defense Robert S. McNamara, who brought to bear mathematically precise ideas of measuring the military effort, especially cost-efficiency techniques in evaluating strategic weapon systems. "The unifying planning-programming-budgeting system installed in the Department of Defense in 1961 has been widely recognized as a major management innovation in the allocation of resources." [24]

Many professional military men were irritated by the implication that computer calculations, operations research, and abstract theories would somehow have greater weight in the decision-making process than military judgment based upon the recorded lessons of history and personal experience. But the history of technology demonstrates the historical trend toward mathematical measurement over personal judgments based on hunch or individual interpretations of the past. To those who might claim that these changes in decision-making techniques are not part of technological history, we must point out that the managerial function has always been extremely important in technology. The pyramids of Egypt, for example, were built not with the aid of sophisticated technical devices or even some very primitive ones, such as the wheel, but were made possible through the efficient organization and mobilization of great quantities of human muscle power. The introduction of new analytical techniques in management forms part of technological history just as much as it does of economic history and, now, of military history.

Injection of systems analysis into the managerial function is indicative of another aspect of the scientific-technological revolution, namely, the growing awareness of the social science parameter in science and technology. Little attention had been paid to such considerations before. Scientists claimed that they pursued their researches without regard for the social affairs of man and mundane applied purposes; engineers disregarded the social purposes of technology unless that purpose expressed itself in economic payoff, and they thought of their work primarily in terms of problem-solving. Systems analysis forces recognition of social and institutional forces operating in the scientific-technological enterprise.

In the case of the armed services, recognition of sociocultural

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[24] Robert N. Grosse and Arnold Proschan, "The Annual Cycle: Planning-Programming-Budgeting," in *Defense Management*, ed. Stephen Enke (Englewood Cliffs, N. J., 1967), p. 24.

parameters antedated World War II, but their importance has been magnified in the postwar era. America's global treaty commitments made imperative more information about the societies and cultures in which our military forces might operate; there had to be propaganda in order to win uncertain peoples over to our foreign policy purposes and military means; and there was the need for counter-insurgency techniques in support of limited-warfare objectives. Furthermore, the military establishment required the social sciences for its own management problems: analyses of manpower requirements and resources; techniques for personnel selection, classification, training, and performance evaluation; and for matters of organizational effectiveness, such as motivation and morale, leadership, command and staff relations, communication, organizational change, and criteria of unit effectiveness.[25]

Another characteristic of contemporary science and technology is the breakdown of old disciplinary barriers. The result is the formation of new interdisciplinary researches, such as material sciences; the abandonment of old classifications of civil, mechanical, electrical, etc., engineering, which grew out of the engineering practice of the 19th century; and the creation of hybrid sciences, such as biophysics, biochemistry, and bioengineering. Similarly, the mission responsibilities of the separate military services broke down following World War II. Prior to and throughout that war, the Army, Navy, and (army) Air Force each had single and distinctive primary missions, even though some secondary mission responsibilities overlapped. Changes in military technology have blurred the former primary-mission separation. The Navy widened its responsibility from simple sea combat to include strategic offensives, first with the carrier air forces and then with the Polaris missile submarines. The Air Force, which concentrated after World War II on long-range delivery vehicles and nuclear weapons, has developed a substantial continental air-defense force and airlift force. Perhaps these overlapping mission responsibilities made it necessary to utilize cost-benefit analysis in order to allocate mission resources. In other words, the managerial innovations were made necessary by changes in strategy which themselves had been dictated by technological changes in warfare.

Another characteristic of our contemporary revolution has been the transformation in the nature and direction of scientific and tech-

[25] Raymond F. Bowers, "The Military Establishment" in *The Uses of Sociology,* ed. Paul F. Lazarsfeld, William H. Sewell, and Harold L. Wilensky (New York, 1967), p. 256. The most celebrated military effort to utilize the social sciences—for studying conditions which might lead to armed insurrection in developing countries—led to a debacle: Project Camelot (1965).

nological activity, and this too has been reflected in military work. Large R&D laboratories, research teams, federal support for science and technology—all characterize our modern scientific and technological activity, and all are part and parcel of military R&D. Although the most quoted portion of Eisenhower's "farewell" address dealt with the "military-industrial complex," in the same speech he also brought out these factors:

> Akin to and largely responsible for the sweeping changes in our industrial-military posture has been the technological revolution during recent decades. In this revolution research has become central. It also becomes more formalized, complex, and costly. A steadily increasing share is conducted for, by, or at the direction of the Federal Government. Today the solitary inventor, tinkering in his shop, has been overshadowed by task forces of scientists, in laboratories and testing fields. In the same fashion, the free university, historically the fountainhead of free ideas and scientific discovery, has experienced a revolution in the conduct of research. Partly because of the huge costs involved, the government contract becomes virtually a substitute for intellectual curiosity. For every old blackboard there are now hundreds of electronic computers. The prospect of domination of the nation's scholars by Federal employment, project allocations, and the power of money is ever present and is gravely to be regarded.[26]

One manifestation of this research revolution has been the development of non-profit institutions for research and analysis. Before World War II practically all the specialized scientific work of the government was done "in house" by government laboratories; 25 years later there were over 350 outside non-profit corporations engaged in this work, the most famous of the "think" organizations being the RAND Corporation, which the Air Force had organized and subsidized to "get the best brains and turn them loose on the problems of the future." Research organizations have been developed by defense contracts and academic institutions as well as the Federal Government. In these R&D establishments, the task force concept of scientific discovery and technological innovation prevails. Some organizations consist of an amalgam of military officers, civilian government officials, and scientists drawn from academia and non-profit institutions as does the WSEG (Weapons Systems Evaluation Group). In 1956 the Department of Defense, viewing the favorable experience of the individual institutions sponsored by the separate services, created a new one, the Institute for Defense Analyses (IDA), set up by a consortium of eight colleges and universities to study problems in disarmament, civil defense, and various weapons systems. The institutionalization of research and development, characteristic of the entire society, has been stimulated by the military's wholehearted endorsement of the concept and practice.

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[26] *New York Times,* 19 Jan. 1963.

The great proliferation of science and technology in the postwar era has enlarged the number of options open for mankind, and this includes the military as well as civilian society. In the recent report of the Harvard Program on Technology and Society, the increasing number of options offered to the individual for his choice was stressed as an important consequence of technological growth.[27] The same holds true for the military. An example is the case of long-range bombardment possibilities. Progression from the B–17 bomber to the B–29 to the B–36 had been straightforward, bigger and faster aircraft simply supplanting their predecessors. But technology offers us the possibility of many quite different strategic nuclear delivery systems, including missiles as well as aircraft, with great variety within each category. We have missiles that can be launched from fixed or mobile land bases, from platforms on the sea's surface or beneath it, and from the air. We have not tried to develop all of these possibilities but we have developed many of them, so that the possibilities of choice have grown, in order to meet every conceivable situation.[28]

We must plan not only for the "last war," which used to be the sole preoccupation of military men, but for problems likely to be encountered under various possible eventualities and contingencies, ranging from prolonged international tension, escalating Cold War, conventional limited wars, to general wars involving nuclear attack. In military technology, even more than in civilian technology, there is an accelerated pace of change. We must be ready for anything and everything—and quickly.

The Interactions of Technology, Strategy, and External Forces

Up to now we have spoken of how the technology of warfare has been intimately related with general scientific and technological developments. However, the interaction between science-technology and warfare has other dimensions, and we have already hinted at the implications of science and technology for strategy. If we pursue this matter a bit further, we can see how the technological revolution has expanded America's military role throughout the world and altered the bases of our national security.

[27] Description of research project of Edward Shils, "Technology and the Individual," in Harvard University Program on Technology and Society, *Fourth Annual Report, 1967–1968*, pp. 23–25.

[28] Alain C. Enthoven, "Choosing Strategies and Selecting Weapon Systems," in *A Modern Design for Defense Decision*, ed. Samuel A. Tucker (Washington, D.C., 1966), p. 135. For a brief discussion of the relations between the social sciences and strategy, see I. B. Holley, Jr., and Theodore Ropp, "Technology and Strategy," in Kranzberg and Pursell, *Technology in Western Civilization*, vol. 2, chap. 38.

During most of our history, geography—the continental insularity of the United States—was the foundation for American security. Even when we felt threatened by outsiders, as in World Wars I and II, we had allies who presumably protected us, specifically the British Navy and the French Army. America's geographical isolation disappeared with the development of aircraft and rocketry. The "absolute security" which we once enjoyed became a thing of the past. The oceans no longer guaranteed the North American continent immunity from military attack, and the military defense afforded by our allies disintegrated during and after World War II. Now, instead of our allies protecting us, our armed might and presence shelter Western Europe and attempt to do the same for Southeast Asia.

Not only did changes in military technology obliterate the defensive values of our insular location, but the requirements of the new technology, demanding raw materials from throughout the world, created vital United States interests in every part of the globe. Our national survival came to be interlocked with those of other free societies. Technology thus brought an end to American isolation in a way which no political ideologies or commitments could.[29]

Furthermore, this changed global role committed us to even greater technological advance. When he was still Vice President and head of the National Space Council, Lyndon Johnson justified the huge space budget of the Kennedy Administration by saying:

> I do not see our survival as a free and first-rate nation unless we lead in space. . . . Visualize, if you will, high-level officials of the world's nations seated about a negotiating table on matters affecting the peace of the world. The nation with the greatest proven competence in space science and engineering would have a huge negotiating advantage over those nations which did not have such strength. If the nation so endowed were to use its space strength to support freedom, the world would gain. If, on the other hand, such nation were one given to blackmail, coercion, and domination, freedom would be the loser.[30]

Military as well as prestige reasons thus combined to push America forward into the great scientific-technological adventure of the space age.

The so-called "space race" illustrates external developments upon the concepts and weapons of warfare. Not surprisingly, the scientific-

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[29] By 1967 the United States had treaty relationships with, or commitments to the defense of, 44 countries and was a member of four alliance systems and a supporter of a fifth. Department of State *Bulletin* 57 (9 Oct. 1967): 460 ff.

[30] Lyndon B. Johnson, "The Vision of Greater America," *General Electric Forum* 5 (July-Sep. 1962): 7-8.

technological parameters of warfare are also affected by these external forces.

When World War II ended, the United States was supremely confident that its gigantic military power embodied in its monopoly of the atomic bomb and its long-range bombers would guarantee world peace. If war did break out, the scenario called for virtually a push-button response. Several factors changed completely this naive view of the nature of future warfare: the apparent determination of the Soviet Union to expand in the countries along its boundaries; the success of the Soviet Union in developing nuclear weapons and rocketry missile systems; and the recognition that aggression could occur by brushfire wars, by insurgency, or by guerrilla warfare, where a nuclear response would be both inappropriate and irresponsible.

The first substantive formulation of postwar American strategy was George F. Kennan's famous article in *Foreign Affairs,* which he signed as "Mr. X." Convinced by his experience in the Soviet Union as a diplomatic officer that the Russian leaders had long-term aims of world hegemony, he believed that the United States could not afford to see the Soviet Union take over one state after another in Eastern Europe and strengthen the Communist parties in other countries to the point where they could seize control and exercise it under instructions from the Kremlin. Kennan therefore advocated that the United States *contain* Soviet expansionism; he felt that this could be done without open conflict with the Soviet Union because the United States possessed a monopoly on the atom bomb.

The Kennan policy of containment, backed by the Truman Administration, brought Soviet expansionism to a halt in Europe through the Truman Doctrine, the Marshall Plan, and NATO. Furthermore, speedy intervention against aggression in Korea showed America's willingness to use its own troops in order to prevent Communist expansion.

No great changes in military technology were necessitated by the policy of containment. Surplus World War II equipment was used to bolster our NATO allies and in the conflict against the North Koreans and their Red Chinese allies; and America's industrial and financial power provided the basis for stemming the advance of Communist parties in the European states through the Marshall Plan.

Nor did the requirements of military technology change greatly when the Eisenhower Administration came into office and John Foster Dulles announced his policy of "massive retaliation" with an eye to stemming future Communist threats to the peace in the Far East.[31]

[31] Speech to Council on Foreign Relations, 12 Jan. 1954, in Department of State *Bulletin* 30 (25 Jan. 1954) : 108.

Instead of attempting to meet the enemy at a time and place of his own choosing, which would require that the United States "be ready to fight in the Arctic and in the Tropics; in Asia, the Near East, and Europe; by sea, by land, and by air; with old weapons and with new weapons . . . ," Dulles called for "a great capacity to retaliate instantly, by means and places of our own choosing." This policy was an attempt to cope with the apparent Soviet technique of not engaging the United States directly but by expanding at peripheral points through local aggression; it threatened the Soviet Union with atomic warfare even for relatively small infractions of the peace.

The doctrine of massive retaliation did not work out as Dulles had anticipated. Although Dulles claimed that the development of small atomic arms—a major technological advance—made possible small-scale retaliation against aggression, no one seemed reassured by a strategic doctrine which would make every war a nuclear war. Furthermore, the American response to Russia's invasion of Hungary in 1956 showed that the United States was reluctant to utilize massive retaliation even on a forthright issue of Soviet aggression.

By the late 1950s Dulles's doctrine of massive retaliation was under attack. In *Nuclear Weapons and Foreign Policy,* published in 1957, Henry A. Kissinger provided the basis for a major change in strategic doctrine. While stating that the United States must retain its ability to retaliate with an all-out nuclear attack if the provocation was sufficient, he also claimed the United States must be prepared to retaliate in more limited fashion against more limited provocations. Kissinger did not suggest abandoning the large nuclear strategic force; he actually added a dimension to the nuclear weapons policy by suggesting the use of small nuclear arms for limited wars. But the primary thrust of his argument was that the United States must be ready to wage "limited wars" with conventional forces. Kissinger's theories became the basis for the key military policies of the Kennedy Administration.

The ability of the Soviet Union to produce a hydrogen bomb and its successful space ventures made it certain that science and technology would be called upon to increase and improve our missile strength. At the same time, the failure of the American-supported invasion of Cuba in 1961 indicated the need to develop a capacity for smaller-scale, non-nuclear conflicts; this was the Kennedy Administration's new policy of "flexible response" to aggression. Such a response had been advocated by General Maxwell D. Taylor, who had resigned from the army in 1959 because of the Eisenhower Administration's refusal to adopt his views on limited war requirements. Taylor had pointed out that small wars posed more threat to American security

than did big wars, and that the United States must be prepared to fight limited wars and to cope with threats of insurgency against free governments.[32]

The combination of Kissinger's and Taylor's ideas—those of the academic intellectual and the experienced military commander—plus American experience with Communist aggression in various parts of the world, including Cuba and the early American involvement in Vietnam, changed the Pentagon's set of priorities and stimulated scientific and technological efforts to cope with problems of limited wars in addition to all-out nuclear war.

The change in strategic doctrine from massive deterrence to a flexible response, designed to meet various kinds of emergencies, can be illustrated by the Air Force's turnabout in developing a long-range transport plane. Prior to the development of missiles, the Air Force goal was very simple: build planes which could fly faster, farther, and higher than those of the enemy. With the development of ballistic missiles and the idea of massive nuclear deterrence, there was little need, it would seem, for a large, long-range transport such as the C–5A. Thus, work on the XC–132, a plane which could carry 500,000 pounds for 5,000 miles, had been cancelled in 1957–58. But the Lebanon and Quemoy crises of 1958 and, later, the doctrine of flexible response forced the revival of interest in long-range transports which could carry troops over long distances and between different theaters of war. Work on a long-range transport recommenced.

Even though flexible response called for the ability for the United States to carry on limited, conventional, and counterinsurgency warfare, that did not mean that the technology to be employed was to be old-fashioned or obsolete. The weaponry employed by the United States in the Vietnam War, for example, is "modern and complex, generally possessing a flexible munitions capability."[33] Furthermore, new dimensions were given to limited warfare, as illustrated by the employment of the helicopter in that conflict. Exploitation of the helicopter has had important consequences for the conduct of land operations, "both as a means of logistic support to the forward area and for the rapid deployment of troops into battle." The area of land which can be dominated by a formation of a given size is now vastly increased, particularly in terrain where communications are deficient

[32] Maxwell D. Taylor, *The Uncertain Trumpet* (New York, 1960).

[33] Harry E. Goldsworthy, "Aircraft Development: Its Role in Flexible Military Response," *Air University Review* 20 (Jan.-Feb. 1969): 21.

and where it would be difficult to construct air strips for fixed-wing aircraft.[34]

Other changes are occurring in military doctrine and training as a result of the flexible response and the technological reaction thereto. Strategic mobility is necessary in order to move troops and equipment to various parts of the globe on short notice; there is the demand for equipment which can function in wide ranges of climatic conditions; and there is a need for highly trained troops who can be quickly deployed to take advantage of the strategic mobility and who can utilize different kinds of weapons in different situations and terrain conditions.

Finally, it should be noted that the flexible military response adopted by the Kennedy Administration and since continued did not decrease scientific and technological efforts in America's nuclear strategic defense policy. On the contrary, both strategic doctrines and military hardware for nuclear warfare have achieved greater levels of sophistication.

Early analyses, in the 1950s, of the strategy of nuclear warfare spoke of first- and second-strike variations, but the objective was the same: destruction of the maximum number of enemy targets, either cities or air bases. These targets were known, they were immobile, and they were "soft" in military terms. They were to be destroyed outright, and little concern was evidenced for collateral damage which might be wrought. Damage to the United States was to be eliminated or limited through the reduction of enemy offensive capabilities either by pre-emption or by quick retaliation following a first strike by the opponent. Penetration of the enemy's defenses was considered a manageable problem, and only a single type of weapon system would be involved.

Advancing military science and technology made these concepts regarding both the objective and strategy of nuclear warfare obsolete. When it was realized that nuclear war can come about without massive initial strikes with little or no warning, a sharp line could no longer be drawn between strategic forces and general purpose forces; hence there was a need to be concerned with damage limitation and with combinations of offensive and defensive systems to attain that objective. Attention was paid to such problems as knocking out a "hardened" target with a weapon of the smallest possible yield, in order to minimize damage to the civilian fabric of Soviet society. Emphasis

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[34] E. C. Cornford, "Technology and the Battlefield," in *The Implications of Military Technology in the 1970s*, Institute for Strategic Studies (London), *Adelphi Papers* No. 46 (March 1968): 47–48.

was shifted from an individual weapon system to a combination of complementary systems. Also, there was a growing recognition that nuclear warfare might come in a sequence of escalating steps from a lower-level confrontation, and this required the meshing of general purpose forces with the strategic forces and making limited war forces part of the mechanism of deterrence.[35]

Secretary McNamara recognized that the strategic problem of nuclear warfare was no longer the simple one of knocking out the enemy's capabilities either before or after we had been struck. We must also "limit damage to our population and industrial capacity." [36] The problem of the anti-ballistic missile defense system thus became a part of America's strategic policy.

Two items are especially to be noted in terms of outside pressures acting upon military doctrines and hence upon science-technology. First is the interaction of America's strategic policy with that of the Soviet Union. Much of American military policy has been dependent upon the actions of the Soviet Union, so that it consists of reaction; and there is a sort of domino theory which carries this on to the science and technology involved in warfare. Second is the importance of the economic element, particularly stressed by Secretary McNamara. Because of the deficiency of information we have regarding the enemy, and also uncertainties regarding the technical values of some of our weapon systems, for example in the case of Minuteman and Titan II, decisions were made on the basis of cost. The missile with the lower initial cost was favored, that is, the Minuteman. Cost was the main consideration which pushed us directly to a force composed of small payload missiles.[37]

But, of course, the main consideration in determining strategy, and hence the applications of science and technology to warfare, has been what the possible enemy might do. The interaction of American strategic policy with that of the rest of the world was tellingly enunciated by Robert McNamara, when he said: "In order to assess the capabilities of our general nuclear war forces over the next several years, we must take into account the size and character of the strategic forces which the Soviet Union and Red China are likely to have during the same period." [38] Secretary McNamara was not advancing any specially new doctrine but was simply applying to the contemporary scene the old

[35] James R. Schlesinger, "The Changing Environment for Systems Analysis," in *Defense Management*, ed. Stephen Enke (Englewood Cliffs, N.J., 1967), pp. 92–94.

[36] Robert McNamara, "The General Problem of Nuclear War," in *Defense, Science, and Public Policy*, ed. Edwin Mansfield (New York, 1968), pp. 7–8.

[37] Schlesinger, "Changing Environment," p. 105.

[38] McNamara, "General Problem of Nuclear War," p. 10.

Latin maxim, *"Si vis pacem, para bellum"* (If you want peace, prepare for war).

Push-Pull Relationships among Science, Technology, and Warfare

Up to now we have spoken only in general terms about the relationships between science-technology and warfare in the post-World War II era. We have pointed out that the military has become increasingly oriented toward science and technology, that warfare has participated—in a mutually dependent relationship—in the contemporary scientific and technological revolution both in military hardware and in the supporting systems, and we have also shown how external considerations have helped modify the directions of strategy and hence of military scientific-technological requirements. Now we must deal analytically with the workings of the relationships between science-technology and warfare.

We start by dealing with an old historical—and philosophical—question: causality. This problem might be briefly stated as which comes first—science-technology or warfare?

When stated that way, the problem sounds like the old chicken-egg question, and that is a futile exercise. We all know that without the chicken there would be no egg; but looked at in another way, the chicken is simply the egg's way of making another egg. In brief, that is a circular process, and which comes first depends on where one begins his analysis of the process.

If the circular process cannot advance our analysis of the relationships between science-technology and warfare, we might turn to the pendulum theory, often used to describe political or other history as alternations between reactionary and radical movements. This pendulum theory might appeal to the military mind, for it corresponds superficially with the alternation of offensive and defensive superiority in warfare. All of you are familiar with the notion, frequently borne out by history, that at times offensive strategy and weapons have forged ahead; eventually defensive techniques and weapons are devised to meet the offensive threat and give effective predominance to the defense; whereupon the offense moves to regain supremacy; and so on ad infinitum. This theory has done valiant service in interpreting the history of past warfare, but I am afraid that it is obsolete today and inapplicable to the relationships between science-technology and warfare. It is obsolete because in a world of sophisticated nuclear devices and missile delivery systems, there can scarcely be a question of defensive or offensive superiority; absolute security has become as impossible

as absolute victory. It is inapplicable because modern warfare is so dependent upon science and technology; these provide boundary conditions for both the offensive and defensive, and while warfare can stimulate the extension of the boundaries of science and technology, it can scarcely advance beyond the constraints placed upon it by the existing level of science-technology.

We must now distinguish between science and technology, which heretofore I have treated as a collective entity. By science I mean the effort to achieve knowledge and understanding of the physical universe; by technology I mean the effort to develop physical means for manipulating the environment for human and social purposes. Science and technology are not unrelated; indeed, the relationships between them are complex and varied, as becomes clear when we investigate their individual and combined connections with warfare.

The stereotyped science-technology relationship is based upon a linear causal relationship which goes somewhat as follows: a scientific discovery leads to a technological application which eventuates in a workable device or technique—in this case, a weapon system or some item of military hardware. In this view, technology is simply applied science. Although this concept follows the facts in a few well-known cases, its simplistic approach is belied by too many other cases to make it a valid generalization.

Figure 1

Science (Basic) $\longrightarrow$ Technology (Applied Science) $\longrightarrow$ Military Hardware

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Figure 2

Science $\xrightarrow{\text{Pull}}$ Military Requirements $\xrightarrow{\text{Push}}$ Technology $\longrightarrow$ Military Hardware

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Figure 3

Military Requirement $\xrightarrow{\text{Push}}$ Scientific Research $\xrightarrow{\text{Pull}}$ Technological Development $\longrightarrow$ Military Hardware

Unfortunately, this false stereotype of the science-technology-warfare relationship (see Figure 1) is embodied in the organization of our military R&D. This process, as described by Peck and Scherer,

consists of four steps.[39] The first, roughly speaking, is basic scientific activity; the second is the search for knowledge about specific means of using the natural phenomena to practical advantage; the third is the identification, modification, and combination of feasible or existing concepts, components, and devices to provide a new application; and the final step is product engineering which results in a weapon system ready for use.

Peck and Scherer claim that most advanced weapons development efforts belong to step three; I wonder if step three has not been the first step in most cases of military hardware development. Indeed, I wonder if their first step—basic scientific research—has formed part of the weapons acquisition process, except in a most indirect and tenuous way. Perhaps much of the muddled thinking about R&D derives from the half-truth that scientific research is the major basis for technological innovation. Most of all, I wonder if the four steps postulated by Peck and Scherer really represent the complex process by which science and technology and warfare interact; an institutional arrangement of the military R&D effort to correspond with this false concept of such interrelationships might hamper scientific and technological advances.

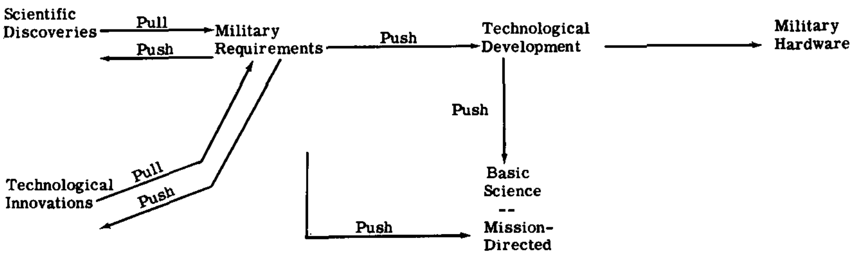
I prefer a more realistic model of the historical relationships which, for lack of a better term, we might call a push-pull model. The pull is exerted by scientific discoveries and technological innovations, with the push being provided by military demands or requirements. This leads us to some interesting model diagrams which can be historically documented.

The first of these push-pull models (Figure 2) exhibits the case of a scientific breakthrough or discovery, which exerts an attraction, or pull, to the military which recognizes the opportunities and potentialities opened up by the discovery or breakthrough. The military then pushes for technological development of this scientific discovery into an innovation which will produce some usable military hardware. Another possibility is shown in Figure 3. Here the military has a clear idea of its requirements or needs, and as a result of these demands pushes upon scientific research to come up with some discoveries, which in turn can be developed technologically into military hardware.

A more inclusive model is shown in Figure 4. Here a scientific discovery or breakthrough, or a technological innovation in some field other than the military, attracts the attention of the military, who can visualize its potentials for warfare. But in the course of the technological development, it is learned that some basic knowledge must be

[39] Merton J. Peck and Frederick M. Scherer, *The Weapons Acquisition Process: an Economic Analysis* (Boston, 1962), pp. 27–31.

Figure 4



secured if any workable items are to result. The result is a push from technology directed toward science in order to obtain the requisite basic knowledge so that there can be development of the military hardware. But changes in one item of military hardware often require changes in other components; the result is another push exerted by the military for specific scientific research or technological innovation which will answer that need. At the same time the military pushes for some undirected scientific breakthroughs or technological innovations that will provide the potential for new capabilities, which will in turn lead to a recognition of new ways to meet military requirements. I think the interactions shown in this kind of diagram best represent the historical facts of the science, technology, and warfare interrelationships during the post-World War II era, and might indeed be applicable to previous periods in history.[40]

One other major point should be made in connection with these models. I believe that the historical data tend to show that the science-technology pull is chiefly responsible for initiating major breakthroughs in military strategy and weapons, such as nuclear explosives and missiles; on the other hand, military requirements for weapon systems exert a push for specific scientific discoveries and specific innovations rather than broad general systems. While there might be some exceptions to these generalizations—for example, the military in terms of ultimate requirements might push for a Buck Rogers-type death-ray gun—this is only done in the most general way; the primary military push on science and technology is for specific details of weapon systems which previous scientific discoveries and technological innovations have brought within the realm of possibility.

[40] T. K. Glennan, Jr., "Research and Development," in *Defense Management*, ed. Stephen Enke, p. 285, discusses push-pull models, but he writes in terms of a military requirement pulling, with technology pushing, for military developments. Leaving aside the semantic problems involved in the use of these words, I think it is more accurate to talk of the pull exerted by scientific and technological developments and the push of military demands, rather than the other way around.

Before we provide historical documentation for our models, we must digress to deal with the question of basic versus applied science. This is crucial, not only in terms of the technological development element of our model, but because the military itself has devoted much research—and some thought—to this question.

Project Hindsight was the name given by the Defense Department to a series of studies of recent science and technology utilized in weapon systems. It was initiated in 1964 to establish the effectiveness of the $10 billion invested by the DOD between 1945 and 1965 in basic and applied research, and to determine, if possible, any management patterns or practices that appeared conducive to a particularly high payoff.[41] Teams of scientists and engineers were appointed to study each of 20 weapon systems; their assignment was to "dissect" the system and identify each contribution from recent (post-1945) science and technology which was clearly important either to improved system performance or to reduced cost. Each contribution was termed an "Event" and efforts were made to identify its cost, source of funds, motivation, and pathway to eventual incorporation into the weapon systems. An Event was defined as a period of creative effort ending with new, significant knowledge or with the demonstration of the applicability of a new engineering concept.

Events were divided into Science Events or Technology Events. Science Events, defined as theoretical or experimental studies of new or unexplored natural phenomena, were further divided into two categories:

> Undirected Science, in which the object of the work is the advancement of knowledge, without regard to possible application, and Applied or Directed Science, in which the object of the work is to produce specific knowledge or an understanding of phenomena which is needed for some particular use or uses. Technology Events includes the conception or demonstration of the possibility of performing a specific elementary function with the use of new or untried concepts, principles, techniques, or materials; the first demonstration of the possibility of performing a specific elementary function with the use of established concepts, principles, or materials; the measurement of the behavior of materials and equipment as required for design; or the development of new manufacturing techniques.

Some 710 Events were identified, with only 9% classified as Science Events. These were distributed as follows: 6.7% were motivated by a DOD need and therefore classified as Applied Science; 2% were motivated by a commercial or non-defense need and were also classified

[41] *The First Interim Report on Project Hindsight (Summary)*, by C. W. Sherwin and R. S. Isenson, was issued by the Office of the Director of Defense Research and Engineering (A.D. 642-400) on 30 June 1966; *Project Hindsight—Final Report, Task I*, by Raymond S. Isenson, was issued on 1 July 1967.

as Applied Science. Only 0.3% of all Events were classified as Undirected Science. 86% of the Events were funded directly by DOD, and an additional 9% by defense-oriented industry. Of the research-performing organizations, industry accounted for 47% of the Events, DOD in-house laboratories for 39%, while universities (including contract research centers) accounted for 12%.

Several conclusions of Project Hindsight are of special interest to us. One was the finding that 37% of the Events which occurred after engineering design was initiated were necessary to the ultimate performance of the system; this explains the importance of the technology push during the development process, requiring more science and technology in order to complete the system. Another interesting conclusion was that the efficiency of production of Science and Technology Events was substantially higher when funded and managed by the Defense Department or defense industry than when funded and managed by the non-defense sector of government or industry. Perhaps most important for our investigations, the systems studied revealed that the contributions from recent (post-1945) research in science were greatest when the effort was oriented. And the authors concluded that the current productivity of DOD in-house laboratories was comparable to that of industry and that the DOD investment in science and technology had had a large payoff.

I do not find it surprising that military officers working on a DOD study should find that the DOD had made a wise investment in science and technology and that the DOD in-house laboratories were as good as or better than those of industry. That is no more surprising than the fact that publication of the interim report raised a great hue and cry among basic scientific researchers, especially from academic scientists. Indeed, so great was the outcry that the final report of Project Hindsight explicitly stated that none of its findings should be interpreted as a disavowal of the value of very fundamental research in science; the findings suggested only that such research is most likely to be utilized when undertaken in a purposeful manner, that is, when deliberate attempts are made to relate the research results to specific military problems.

It is not surprising that the National Science Foundation, responsible for the funding of basic science, underwrote an investigation by the IIT Research Institute, beginning in 1967, for a systematic study of the role of basic scientific research in the overall process leading to technological innovation. That report, *Traces (Technology in Retrospect and Critical Events in Science)*,[42] can be regarded as academia's

[42] Prepared for the National Science Foundation by the Illinois Institute of Technology Research Institute under contract NSF–C535. Vol. 1 was published 15 Dec. 1968.

reply to Project Hindsight. Key scientific events which led toward five major technological innovations were traced. Unlike Project Hindsight, *Traces* did not deal with weapon systems but with other developments of social and economic significance: birth control pills, the electron microscope, video tape recording, ceramic-metallic materials, and matrix isolation. Instead of setting a backward time-limit of 1945, *Traces* went back more than a century in studying the scientific roots of certain innovations. Dividing its key events into non-mission research, mission-oriented research, and development and application, *Traces* discovered that non-mission events amounted to approximately 70% of the total, 20% to mission-oriented research, and 10% to development and application. Furthermore, the number of non-mission events peaked significantly between the 20th and 30th year prior to an innovation, whereas Hindsight found a delay of five to ten years between the DOD investment in research and the payoff.

I do not find that the conclusions of *Traces* and Project Hindsight are contradictory. Had Project Hindsight looked further back in time or wider afield than weapon systems, its conclusions might have been much closer in percentages to those of *Traces,* while *Traces* might have come up with a somewhat different set of percentages had it chosen a different set of innovations, particularly some involving mechanical rather than chemical, biological, and electronic devices. They agree that both fundamental and applied research play roles in innovative activity and that mission-oriented research becomes increasingly important as the time comes closer to the final innovation. The importance of this latter point to our study lies in the fact that even when some basic scientific discoveries have opened up vistas of military exploitation, and even when the project is in the stage of technological development, it frequently becomes necessary to return to the laboratories for some basic research—even though it is mission-oriented—and this helps explain the push-pull relationship between science and technology themselves.

Examples of scientific breakthroughs and technological innovations providing the pull for military developments are familiar to all of us. In this setting, it is appropriate to recall that the air parameter of warfare arose from a civilian achievement. Only after the Wright brothers' curiosity and experiments had shown that heavier-than-air flight was possible did the military display much interest. This is not to deny that many of the subsequent developments in aircraft arose from the military push, but the great breakthrough itself—the advent of powered flight—was independent of any military concerns.

The most striking example of the scientific pull in recent times arose from basic scientific investigations of the atom. The story is so well

known that it scarcely needs retelling. Certainly the military uses of atomic power were scarcely uppermost in the minds of Einstein, Fermi, and others when they embarked upon their scientific investigations, and the military was relatively slow to respond to the possibilities of nuclear fission. When Fermi spoke at the Naval Research Laboratory on March 16, 1939, only a few scientists displayed interest, and they were chiefly concerned with a source of power that would permit protracted underseas operations by freeing submarines from dependence on tremendous supplies of oxygen. Even after Einstein's famous letter to President Roosevelt in the summer of 1939, the military was slow to act, and it was finally the civilian scientists of the National Defense Research Committee who pushed the project and began real work on it.[43]

Rocket missilery, the second pillar of the 20th-century military revolution, was also the product of civilian investigations. Robert H. Goddard in this country, Konstantin Tsiolkovskii in Russia, and Hermann Oberth in Rumania—the pioneers in rocket development—were motivated by scientific, not military, considerations. Goddard, for example, was supported throughout the 1920s and the 1930s by the Smithsonian and Carnegie Institutions and the Guggenheim family; it was not until some time after he had successfully demonstrated liquid-fueled rocket flight that the military services became interested in his work.[44] Of course, once the military finally recognized the warfare potentials of nuclear fission and rocketry—and it was the Germans who demonstrated the latter to us—they pushed forward their development into revolutionary weapon systems. It is important to remember, however, that the fundamental bases of our security rests on these two revolutionary developments which were the outgrowth of basic scientific research and technological innovation. On the other hand, the military is much more alert to the potentialities opened by basic scientific research now than it was at the time of the Wright brothers or Robert Goddard or Albert Einstein.

The military push for scientific discoveries and technological innovations seems almost too obvious to require elaboration. The whole history of nuclear weapons and rocket missilery, the two foundations of our contemporary military revolution, illustrates how much further research and experimentation were necessary before these major breakthroughs were translated into workable devices.[45]

Nevertheless, Solly Zuckerman, the chief scientific advisor to the

[43] Hewlett and Anderson, *New World, 1939/1946,* pp. 12–26.

[44] See Eugene M. Emme, ed., *The History of Rocket Technology* (Detroit, 1964), pp. 5–6.

[45] See Hewlett and Anderson, *New World, 1939/1946,* chaps. 5–7, 9; and Emme, *Rocket Technology,* chaps. 5–8.

British government, has expressed doubts that the progress of scientific knowledge is accelerated under stimulus of military need. Mr. Zuckerman, it seems to me, is both right and wrong. No major—and I stress the word "major"—scientific discovery or technological advance seems to have been derived from the push of military requirements. On the other hand, a host of minor, albeit significant, advances have come about as a result of the military push. Thus, as we have indicated above, the advent of powered flight did not come about as a result of a military demand; but one can trace much of the progress after the Wright brothers to military support of aircraft development. Similarly, the military stimulus to space flight has been extremely important, once the breakthroughs had occurred. We can cite other examples of the military push for scientific and technological advance; the desire to utilize certain characteristics of electronic circuitry for missile and inertial guidance systems has been productive of many important fundamental researches and technical innovations, such as the miniaturization of electronic devices. To take a lesser known example, scientific investigations into the characteristics of beryllium have been stimulated as a result of its possible use as a structural material for space and aircraft applications.[46]

Rather than belabor the obvious by a long list of technical innovations and scientific researches which came about through the military push, it might be more interesting—and more amusing, if I were not a taxpayer—to mention an instance where science and technology failed to respond to the military push. This case is all the more interesting because it runs counter to the public belief—shared by the military—that contemporary science and technology can meet all the demands placed upon them. It is a case familiar to all of you, I am sure: the TFX, or the F-111. All of us remember the great controversy of 1963 evoked by Secretary McNamara's insistence that a single all-purpose plane could satisfy both Navy and Air Force needs. Neither service was enthusiastic about the idea, because each had its special requirements. Even in theory and blueprint form, it was difficult to produce designs acceptable to both services, and a further hassle arose when the development contract was awarded to Convair. But McNamara stood his ground; if he could get a single plane to meet the requirements of more than one service, he would save almost a billion dollars and would establish a precedent which would effect great savings in other weapons.

The result, of course, has been a fiasco. Despite a number of tradeoffs among weight, speed, size, firepower, etc., the finished product could not be used by the Navy, which found it too heavy for its carrier decks,

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[46] Solly Zuckerman, *Scientists and War: the Impact of Science on Military and Civil Affairs* (New York, 1967) ; *NAS News Report* (National Academy of Sciences) 19 (Feb. 1969).

and even the Air Force had to ground its F–111s in Vietnam after a succession of crashes. The TFX story, wholly apart from the controversy between the services and over which company would win the contract, indicates that science and technology, despite an extremely strong military push, could not produce a plane which possessed the "commonality" which McNamara insisted upon.

Secretary McNamara had left office before the failure of the F–111 venture became apparent, but his application of cost-benefit analysis to the TFX problem faces us with a major question in analyzing the military push on science and technology: How does the cost-benefit approach to weapon systems development affect scientific and technological advance? I know of no study which has investigated this question. Yet, planned-program-budgeting, which forms part of the cost-effectiveness system installed by McNamara in 1961, has undoubtedly affected the allocation of resources to specific areas of science and technology.

Charles Hitch, former comptroller of the DOD and one of the prime advocates of programing and cost-effectiveness techniques, has pointed out that it is only when weapon systems have reached the stage of advanced development that cost and possible benefits begin to be considered, and hence cost-effectiveness studies do not act as too sharp a brake on the innovation process. Yet Christopher Hartley has pointed out that considerations of cost rather than technical feasibility led the United States to invest in Minuteman and Polaris rather than Skybolt, and there are probably other examples of this type.[47]

On the other hand, a case might be made that the emphasis on costs has perhaps stimulated a search for alternative technologies, just as similar cost competition in private industry has been productive of technological innovation. At the same time, the enormous cost of today's weapons, the long lead time necessary to produce complex weapons, and the wide choice of weapons available through advanced technology would seem to make imperative some form of budgetary analysis in allocating resources for science and technology. Until we have close and objective investigations of actual cases, we cannot determine whether the present system of cost-benefit analysis has stimulated or hampered discovery and innovation.

We also cannot tell whether military R&D should be divorced from production, and performed in non-profit research institutes and govern-

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[47] Charles Hitch, *Decision Making for Defense* (Berkeley, 1966), reprinted in Mansfield, *Defense, Science, and Public Policy*, pp. 79–95; Hartley, "Future of Manned Aircraft," p. 28.

ment laboratories rather than in defense corporations or DOD in-house laboratories. Dr. Carl Kaysen, Director of the Institute for Advanced Studies, argues to that effect, although Project Hindsight points out that defense industry can play a major role in R&D, and the amount of learning transferred between the production line and the R&D component would seem to argue against Kaysen's position of separating the two.[48] Again, more case studies are needed to determine the effectiveness of the scientific and technological stimulus in the R&D laboratory as compared with the production stimulus.

Like most historians I tend to concentrate on new and revolutionary developments, the dramatic scientific breakthroughs and spectacular technological innovations, rather than concerning myself with less dazzling but nevertheless important developments in the older branches of warfare. But these must not be neglected, for they exhibit the same military push on science and technology as do the more exotic and newer ballistic missile systems. Actually, very important advances have occurred in the technology of ground warfare to make it very different from, say, the infantry combat of World War II or the Korean War.

For example, science and technology are altering the balance in the continuing battle between the armored fighting vehicle and the weapons to counter it; at the moment the balance favors the anti-tank weapon. Since World War II, we have developed short-range recoilless weapons and long-range anti-tank guided weapons carrying a shaped charge capable of inflicting considerable damage after penetrating heavy armor. These anti-tank guided weapons no longer need rely on simple line-of-sight guidance systems, but on television or infra-red tracking. Advances are also being made in battlefield surveillance and night combat techniques, and here both science-technology pull and military push have entered the picture. Radar had already enabled air and sea warfare to be conducted almost as efficiently in darkness as by daylight, but it had made little contribution to land combat because of the complexity of the radar picture of the landscape and the difficulty of discriminating military objects from the natural background. But now battlefield surveillance radars, exploiting the Doppler effect on returned signals from moving objects, enable the movement of men and vehicles to be readily observed. Improvements in thermal and acoustic detection techniques have also been made—and applied in combat in South

[48] Carl Kaysen, "Improving the Efficiency of Military Research and Development," in Mansfield, *Defense, Science, and Public Policy*, pp. 114–15. Samuel Hollander, *The Sources of Increased Efficiency: a Study of DuPont Rayon Plants* (Cambridge, Mass., 1965), argues that the technical staff at the operating level proved more significant in effecting technical changes to improve efficiency than did the formal R&D group, which was divorced from the production process.

Vietnam.[49] Improvements in helicopters also testify to the effectiveness of the military push in impelling technological advance.

But, it should be noted, the Army, no less than the Navy and Air Force, has had its fiasco in military technology. The deficiencies of the M–14 and M–16 rifles are evidence of the failure to apply science and technology effectively to the small arms problem. True, with the emphasis on nuclear war after World War II, the R&D budget for small arms development was drastically cut after 1945. Although the Army could afford to exert only a weak technological push, that might have proved adequate had not the Army, through a combination of blind conservatism, misguided patriotism, and the inept application of costing techniques, failed to take advantage of its own R&D work which could have produced superior automatic weapons for our infantry.[50]

As in the case of the TFX, the Army set down specifications for a lightweight automatic rifle that proved technically impossible to meet. Small arms designers could not produce a weapon which would possess desired characteristics of weight, reliability, and accuracy and still employ the 30-calibre ammunition which the Army required partly for reasons of economy and tradition. The Army also disregarded the scientific study of the effectiveness of small-arms fire made by its own contract researcher, the Operations Research Office (ORO), and persisted in demanding a weapon and ammunition which would be extremely accurate at 1000 yards. The ORO had shown that small arms were rarely fired at that range, but the Army was reluctant to change its specifications and adopt a rifle design which would have been both feasible and optimal in terms of actual combat experience.

Finally, we must not overlook the push toward technological developments arising from developments in related technologies. For example, technical developments in one field can affect other components or an entire weapon system. "The extraordinary progress that has been made in reducing the weight of fission and fusion weapons, for example, has had a very considerable influence on determining the preferred kinds of missiles." [51]

Changes in one basic element of a weapon system can change the entire system, and since certain components can undergo rapid and striking advances, systems are constantly changing. Computers provide

[49] Cornford, "Technology and the Battlefield," pp. 48–54.

[50] Edward C. Ezell, "The Search for a Lightweight Rifle: the M–14 and M–16 Rifles," Ph.D. dissertation, Case Western Reserve University, 1969.

[51] Burton H. Klein, "Policy Issues Involved in the Conduct of Military Development Programs," in *Economics of Research and Development*, ed. Richard A. Tybout (Columbus, Ohio, 1965), pp. 318–19.

an excellent example of this. They are critical components of weapon systems—part of airborne inertial guidance systems that keep offensive missiles pointing at their targets, and part also of the radars and defense missiles that might be used to shoot them down. But the computer art has changed at an extraordinary rate; computers multiply in speed, go down in price, increase in reliability, and shrink in size.[52] Computer improvements are themselves the result of fundamental advances in the applications of solid-state physics and micro-electronics. Order of magnitude improvements come very rapidly in the tiny elements that form essential parts of computers and sensing and communications systems, making military offense and defense systems ever faster in their reactions and ever more effective.[53]

One of the significant characteristics of military development projects is that the technology constantly changes during the project. "It is seldom indeed that the differences between the system as it was initially conceived and as it emerges from development are only a minor sort," Burton Klein has stated. "For example, the Congressional hearings on the missile programs show that almost all the major subsystems now being used in the Atlas missiles are of a different kind from those initially planned. . . . In fact, a reasonable operational definition of a missile system would be that it is a system mainly made up of components and subsystems initially developed for other missile systems." [54]

Qualitative Differences between Military and Civilian Science-Technology

I have earlier suggested that the science-technology-warfare interactions following World War II are qualitatively different from those of any previous historical period. One reason for this can be seen in our push-pull models. While the military push for technological innovation can be shown to have existed back in antiquity—and perhaps in prehistory if we had sufficient evidence—the scientific-technological pull during and after World War II represents, broadly speaking, a new factor in the historical equation.

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[52] See Paul Armer, "Computer Aspects of Technological Change, Automation and Economic Progress," in *Technology and the American Economy* (report of the National Commission on Technology, Automation and Economic Progress), vol. 1, *The Outlook for Technological Change and Employment* (Washington, D. C., 1966), Appendix, pp. 205–32.

[53] Albert Wohlstetter, "Strength, Interest, and New Techniques," in *The Implications of Military Technology in the 1970s,* Institute for Strategic Studies (London), *Adelphi Papers* No. 46 (March 1968): 3.

[54] Klein, "Policy Issues," p. 311.

In antiquity the military push undoubtedly resulted in many advances in mechanical devices and in metallurgy for the production of weapons. With the introduction of gunpowder, military requirements wrought profound changes in metallurgy, machine tools, and chemistry. The military push was felt far afield, as witness the invention of food canning by Appert in 1810 in response to the needs of the French army.

Evidences of the science-technology pull on the military are relatively rare before recent times, however. The wheel probably found use for transporting goods before it was used on the war chariot. The stirrup—which was to effect such a profound transformation in military tactics and strategy [55]—also seems to have acquired its military application following its civilian use. Other transportation devices, for example, the truck and the airplane, represent a similar science-technology pull which the military later exploited. These—and others which I have not mentioned—represent significant exceptions to the generalization that the science-technology pull on the military was weak or rare before World War II. Yet I believe that the generalization remains valid, for not until the past quarter-century do we find the military constantly surveying scientific discoveries and technological innovations to determine their possible applicability to warfare. The quantitative change here is so great as to make it qualitatively different from the previous period.

However, the qualitative changes which I wish now to stress are the differences between science-technology for the civilian world and science-technology for warfare. These differences center on two major facts, namely, the change in the amount and nature of the "spillover" from military science and technology to the civilian sector of society, and second, the creation of a military industry in our country after World War II which had no real counterpart in prewar America.

Just as Thomas Jefferson equated the citizen with the soldier during the early days of our Republic, so there was little difference between civilian and military technology for most of American history. The skills demanded of the soldier during a less sophisticated period of warfare were very similar to the skills of the citizen living in a predominantly rural society. During the early period, muskets, cavalry accoutrements, and artillery pieces were the prime elements of military materiel, and with the exception of the latter, these were also useful civilian articles in a frontier-minded society where hand weapons were common household equipment on a par with plows and livestock. Military transportation called for horses and mules, and these also fulfilled civilian trans-

[55] Lynn White, Jr., *Medieval Technology and Social Change* (Oxford, 1962), chap. 1.

portation needs, as did, later, the railroads. Virtually every item of military equipment had some civilian use, and vice versa, so there was almost complete exchange of military and civilian technology.[56]

Innovations in methods and tools for making war material could also be utilized in civilian technology. For example, even though Eli Whitney might not have been responsible for the development of interchangeable parts, the fact is that the concept of interchangeable parts which made possible modern mass production was first applied in the production of small arms for the military. Furthermore, United States arsenals pioneered in the development of machine tools and metal-working techniques, which were readily transferred to civilian industry.[57]

Thus, for most of American history, the products, machines and equipment, and processes of military technology were almost directly applicable to civilian technology. Even during this century, advances in military aircraft were almost directly and immediately applicable to civilian aircraft. This situation has changed in the postwar era. The growing specialization of military needs and the scientific and technological sophistication of military equipment and production techniques have militated against easy transference from military to civilian technology.

True, there is still some direct product adaptation. Research on mildew resistant fabrics for military use in the tropics obviously has civilian application, but this type of research is only a small percentage of military R&D in today's missile age.[58] Only a small proportion of the products of contemporary military science-technology can be directly appropriated by the civilian sector.

Some products of military science-technology can be indirectly appropriated, however.[59] For example, the development of nose cones

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[56] Neil H. Jacoby and J. A. Stockfisch, "The Scope and Nature of the Defense Sector of the U.S. Economy," in *Planning and Forecasting in the Defense Industries*, ed. Jacob A. Stockfisch (Belmont, Calif., 1962), reprinted in Harry B. Yoshpe, ed., *Production: the Industrial Sector in Peace and War* (Washington, D.C., 1966), pp. 133–34.

[57] Robert S. Woodbury, "The Legend of Eli Whitney and Interchangeable Parts," *Technology and Culture* 1 (Summer 1960): 235–53; Nathan Rosenberg, "Technological Change in the Machine Tool Industry, 1840–1910," *Journal of Economic History* 23 (Dec. 1963): 414–43.

[58] Richard R. Nelson, "The Allocation of Research and Development Resources: Some Problems of Public Policy," in Tybout, *Economics of Research and Development*, pp. 291–92.

[59] An excellent brief discussion of the transference problem is in Richard S. Rosenbloom, *Technology Transfer—Process and Policy*, National Planning Association, Special Report no. 62 (Washington, D.C., 1965).

for space vehicles is said to have resulted in items for civilian consumption—although it is a moot point if the Corningware coffeepot owes very much to the ceramic nose cone. Nevertheless, certain military components or developments, such as the miniaturization of electronic devices, can be utilized in civilian products.

It has been argued that military technology has had an impact on civilian industry by encouraging the emergence of new technologies such as electronics. In addition, these new technologies enable existing industries to develop a new range of equipment, instruments, and materials that are replacing, improving, or extending old types of production. Yet, much of the new technology stimulated by military requirements has not been widely adopted by civilian industry. For example, large aerospace companies which specialize in producing for the military "obtain only one or two per cent of their sales from products based on their defense/space work that are sold in commercial markets. The list of abandoned commercial ventures is long, ranging from stainless-steel caskets to powered wheelbarrows, to garbage-reduction machinery." [60] It would appear that military technology no longer has the direct impact on civilian technology that it had in the years before World War II.

There are reasons for this difference. I have mentioned some of them, including the highly specialized nature of today's military requirements and the sophistication of modern weaponry and equipment. But there are other differences. One is the time factor. Military science-technology works under pressure—in order to meet competitive scientific and technological advances by the Soviet Union in military hardware—and civilian industry does not labor under such pressures.

There also seems to be a higher degree of uncertainty in military R&D than in civilian science-technology.

> There is uncertainty about the future detailed objectives of our military forces, about the future effectiveness of these forces, and about the alternative means available for achieving these objectives. . . . There are many internal uncertainties also. Will a particular technological approach work as predicted? Will the components integrate together without serious interference? Will the sub-systems be sufficiently reliable to permit the achievement of mission objectives? [61]

Because of the long lead-time involved in developing advanced weapon systems, their effectiveness depends upon events which may be five to

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[60] Murray L. Weidenbaum, "Defense Expenditures and the Domestic Economy," in Enke, *Defense Management*, pp. 328–29.

[61] Glennan, "Research and Development," p. 276; see also Peck and Scherer, *Weapons Acquisition Process*, chaps. 2, 11.

twenty years in the future. Another source of uncertainty is the possible emergence of new technologies which provide alternative means of achieving the same goal. Thus the Air Force's cruise-missile program, involving the Snark and the Navaho, was overtaken by the ballistic missile before either of these had achieved any useful operational capability. This is not to deny the uncertainties of scientific and technological advance in civilian industry. But the stakes are not so large, and civilian industry is hesitant to commit itself to new technologies—the annual model change, as we all know, brings very little real change in the product itself.

The fact is that military science-technology, insofar as it concerns the weapons acquisition process, seems to be unique. Peck and Scherer, who have investigated this question, reject the market system and commercial product development analogies to the weapons acquisition process, which, they claim, does not possess the salient characteristics of commercial activity on the one hand or of scientific activity, in terms of organizational and administrative concepts, on the other.[62] One major difference, of course, is that the military places greater emphasis on time and quality considerations than on cost reduction. The economic factor, even with the application of cost-effectiveness techniques, is not the prime factor in the military's consideration of scientific and technological development, and that alone is sufficient to distinguish it from civilian technology.

Another distinctive characteristic of military science-technology is that it requires a higher concentration of scientists and engineers than civilian industry.

> The typical company or division of a company specializing in defense and space work hires four or five times more scientists and engineers than the most technically-oriented commercial company to support the same volume of sales. For a typical company producing aerospace systems, engineers and related technical personnel no longer constitute a single important but limited department. They may exceed in actual numbers the total of factory or "blue sollar" employment. In large measure, these companies have become primarily aggregations of R&D resources.

Aircraft and missile companies alone employ more scientists and engineers on research and development work than the combined total of the chemical, drug, petroleum, motor vehicle, rubber, and machinery industries. Weidenbaum estimates that about 52% of all the scientists and engineers doing R&D work in American industry are engaged on projects funded either by DOD or NASA.[63]

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[62] Peck and Scherer, *Weapons Acquisition Process,* chap. 3.
[63] Weidenbaum, "Defense Expenditures," p. 323.

The higher concentration of scientists and engineers in military science-technology provides an interesting new possibility for the transference of technology from the military to the civilian sector. Such transference might no longer consist of products, machines, or processes, but rather the transfer of expertise through the movement of personnel. To put it differently, military science-technology develops scientists and engineers for the private economy. We have some limited data on the movement from defense to private employment. For example, numerous veterans are now using skills, such as those in electronics, that were learned in the military service. (Over 16% of enlisted-personnel separations from the armed services during the period 1957–63 were trained in electronic skills.) Another "example of the movement of defense-industry personnel to civilian work occurred as a result of the Dyna-Soar cancellation: two-thirds of the laid-off employees found jobs in non-defense fields." [64]

The demand for scientists and engineers engendered by defense needs has widened the job market and helped to produce scientists and engineers for the civilian economy as well as for defense programs. In a sense, this marks a return to the 19th-century situation when our military academies produced engineers who contributed to the industrial expansion of this country. Now, by their concentration on science and technology, the military services are encouraging the production of scientists and engineers who help raise the scientific and technological level of society.

The qualitative differences between civilian and military technology following World War II are shown not only by a change in the nature of the transference process but also by the emergence of a new sector of the economy: the defense sector, or the military-industrial complex. Again, the growing specialization of military weapon systems and the science-based nature of defense industry are responsible for this fact.

As I have indicated previously, the requirements of both civilian and military technology in an earlier period of our history were quite similar. As President Eisenhower put it in his "farewell" address, "American makers of plowshares could, with time and as required, make swords as well." To be sure, there were some "munitions makers," firms that produced gunpowder, and armor plate. With the exception of those producing armor plate, these industries did not employ any unique technology. Although much attention was lavished on the "merchants of death" during the 1930s, the fact is that most industries catering to military needs used the same tools and techniques to produce

[64] *Ibid.*, p. 324.

similar civilian products. The one exception was naval construction, which required armor plate, a product not particularly useful for civilian society; and, when the torpedo and submarine appeared, they required a specialized technology that made naval construction a prototype of later defense industry.[65]

Not until the massive military effort of World War I was American industry required to produce specialized defense products on a huge scale. However, major emphasis was placed on production management and techniques in order to enlarge the output of familiar goods rather than on new product development, so little science and technology were involved. Their experience with defense contracts during World War I and its aftermath disillusioned many corporations; during the war these industries accepted large defense contracts only to suffer dislocation and hardship when these contracts were abruptly cancelled at the end of hostilities. As a result, business was reluctant to take on military orders again in 1939 and 1940—until the United States was actually in World War II, at which time industry cooperated wholeheartedly in the production of military goods and equipment.[66]

In the two decades after World War I, military technology advanced mainly by drawing upon automotive and aircraft progress. In other words, the technology of warfare advanced primarily by feeding on advancing civilian technology. Improvements in metallurgy, chemistry, machinery, and the like, though sometimes stimulated by a military push, were largely directed at civilian needs, and the military adapted these advances for its own purposes. Perhaps the only major innovation introduced by the military in the period between the two World Wars was the aircraft carrier, which revolutionized naval tactics and strategy.

World War II changed all that, although the transformation was not apparent at first. Indeed, the war began by being fought and managed along World War I lines, emphasizing mass production and standardized equipment, and the standard military dictum was that no new weapons would appear unless these were already on the drawing boards at the outset of the war. Yet during the course of the war there emerged the purposeful application of scientific and technological research to military problems, and the coming into being of a revolutionary new weapon. Nevertheless, the rapid demobilization immediately after World War II pointed to a repetition of the post-World War I pattern; it seemed as though defense industry was again to be a temporary

[65]Jacoby and Stockfisch, Scope and Nature of the Defense Sector," quoted in Yoshpe, *Production,* pp. 135–36.

[66] Huntington, *Soldier and the State,* p. 364.

phenomenon, appearing during times of national crisis and then disappearing almost immediately after the cessation of warfare.

The turning point, marking the emergence of a defense industry as a permanent feature of the American economy, was the outbreak of the Cold War, or perhaps more specifically, the Korean War. It became clear that America could not afford to let down its military guard. Thus a major element in bringing about the rise of a defense industry was the fact that such industry was assured of a substantial and continuing market, the U.S. military establishment. There was now a constant military push for an industrial response, which took the form of large sectors of the industrial economy being directed toward meeting warfare requirements, even in peacetime.

This meant an ongoing defense industry, not one which appeared only in time of war or which, after a war, provided simply for maintenance of specialized plant equipment, machine tools, and strategic raw materials on a standby basis allowing for rapid conversion to arms production in case of war. Furthermore, our global commitments led to a policy of foreign military assistance, and this too helped create a steady demand for the products of defense industry. In other words, the mere volume of military business, and its continuance in time of peace, was a powerful stimulus to the creation of defense industry.

Another factor in the creation of a specialized defense industry was the complex technological requirements of modern armed forces. It is true, of course, that one portion of the defense industry consists of large manufacturers which produce military items while at the same time catering to the civilian market. An example is the automotive industry. During the peak army ordnance procurement for the Korean War, July 1950–June 1953, General Motors was the number one military contractor based on size of orders received, because it was a major producer of tanks and trucks. However, by fiscal year 1964 General Motors had fallen to 19th position in military contracts, and the fact is that the bulk of General Motors' business, even during the period of peak supply for military needs, was for the civilian market.[67]

But attention must be directed to a wholly new type of industry, particularly in such fields as aerospace and electronics, which is almost totally dependent upon military orders. What we have here is an entirely new phenomenon: defense industry as a continuing part of the American economic and social scene. This military-industrial complex is a new factor in American history.

Reliance upon one customer, and that a customer whose primary

[67] Weidenbaum, "Defense Expenditures," p. 321.

interest is in the constantly changing specialized requirements for warfare, makes defense industry differ greatly from its civilian counterpart. Furthermore, defense industry must possess a high degree of specialization of skills and facilities because the military continually thinks in terms of future systems that require improvement in the state of the art. Thus the military push for scientific and technological advance is felt more keenly in defense industry than in other segments of the economy.[68]

The military-industrial complex reverses some previous trends in American history. During the last half of the 19th century, business had been almost universally hostile to the military. The military in its turn disliked commercialism and felt itself alienated from American business society. Now, however, defense industry supports for economic reasons the same military policies which officers support for professional reasons, so for the first time in American history, military programs possess significant economic support.[69] After World War II, the military entered into close association with the business elite of American society, and it is precisely this identity of interest and the power associated with it that President Eisenhower warned against.[70]

Another characteristic of contemporary defense industry is the almost-handcrafted nature of its products in comparison with the mass production output of civilian technology and pre-World War II military technology. So complicated and sophisticated are today's weapon systems and their components that they do not lend themselves to the techniques of mass production for standardized items. The care and skill required to assemble, say, complex electronic devices for nuclear missile warheads calls for quite different techniques than a production line for ordinary artillery shells. Hence the manufacturing techniques for some very significant military items differ from military production before World War II.

The nature of the product and the highly sophisticated and complex requirements of the military are perhaps responsible for another distinguishing characteristic of defense industry, namely, its great reliance upon the "knowledge industry." "What firms in the defense sector sell primarily is a qualified organization of people who, combined, possess a peculiar 'know how' related to a technology useful in

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[68] Jacoby and Stockfisch, "Scope and Nature of the Defense Sector," quoted in Yoshpe, *Production*, pp. 137-39.

[69] Huntington, *Soldier and the State*, pp. 226-27, 268, 266.

[70] For an indictment of the military-industrial complex and its effect on science, see Harold L. Nieburg, *In the Name of Science* (Chicago, 1966).

operations." [71] Its dependence upon advanced scientific knowledge and technical expertise makes the Department of Defense extremely sensitive to the need for maintaining goodwill among the scientific and engineering communities, for supporting basic as well as applied research, and for training future scientists and engineers. In a letter to the *New York Times* of April 2, 1969, Dr. Lee A. DuBridge, Science Advisor to President Nixon, attempted to reassure the academic community regarding DOD's concern for pure science and for academic freedom:

> I do not agree with those who say that universities should not accept any support from the Department of Defense. Many agencies within DOD have for many years supported basic research without any foreseeable relationship to weapons and without any restriction on full publication of the result. This research is of the sort that the university itself thinks appropriate and educationally valuable. This is fine, and I hope such support from DOD will continue.

The fact is that the DOD and NASA finance about three-fifths of all research and development performed in the United States, far surpassing in dollar significance the R&D funds supplied by all other sources, including private industry, colleges and universities, and other non-profit institutions.[72] It is interesting to note, also, that the government outlay for university research represents two-thirds of all the research money which the universities have available.

Defense science and technology have thus become of major significance for American society, not only because of the magnitude of their expenditures and their importance to our nation's defense, but also because they are vital to support of research and education in science and engineering. As a corollary, because the defense program utilizes a major share of our scientific and engineering talent and supports so much of the R&D effort, it thus plays a large role in shaping the course of scientific and technological advance.[73]

The federal government, especially for defense purposes, has become the Maecenas, the patron, of science and technology, displacing the university, industry, and private foundations. Furthermore, it has fashioned a wide variety of institutions to administer its vastly increased commitment for scientific and technological excellence.[74]

In order to understand the full significance of this, in terms of the

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[71] Jacoby and Stockfisch, "Scope and Nature of the Defense Sector," quoted in Yoshpe, *Production*, p. 140.

[72] Weidenbaum, "Defense Expenditures," p. 319.

[73] Mansfield, *Defense, Science, and Public Policy*, p. vii.

[74] Krishnan D. Mathur, "Science and the Federal Government," in Sanders and Brown, *Science and Technology*, pp. 85–86.

military-science-technology interrelationship, we should remember that before 1940 practically all the specialized intellectual work of the government was done "in-house" by government laboratories. By 1964 some 350 outside, non-profit corporations were assisting the government, and if we count the support given by the Defense Department and other government agencies to universities, the number is far larger. At the same time, the DOD "in-house" R&D laboratories have grown in size. About one hundred thousand civilians, one-third of them professionals, worked for DOD laboratories in 1963.

While it is difficult to make value judgments and comparisons between the quality of pre- and post-war military science and technology, there would seem to be little difference today between the levels of civilian and military science-technology. Perhaps the chief reason for this is the constant "cross-fertilization" among military laboratories, defense industries, and university faculties. This kind of contact and variety of experience simply did not exist before World War II.

Perhaps another reason for the present high quality of military scientific and technological research is the development of the task force system which combines military and civilian talents to achieve a specific goal. The Manhattan Project during World War II served as the prototype for the Special Projects Office in the Navy that produced the Polaris, and it has provided a model for the development of many weapon systems since then. In his "farewell" address Eisenhower noted somewhat ruefully the fact that "task forces of scientists in laboratories and testing fields" were replacing "the solitary inventor, tinkering in his shop." He was merely recognizing a fact made necessary by the complex and specialized nature of today's science and technology.

In that same address, President Eisenhower spoke of another related facet of contemporary scientific and technological research: "Partly because of the huge costs involved, a government contract becomes virtually a substitute for intellectual curiosity. For every old blackboard there are now hundreds of new electronic computers." As usual, Ike evinced his concern for any extension of the government's role in society, as well as his own nostalgia for the past. Characteristically, he somewhat misstated the problem.

Government contracts for research and development, far from becoming a substitute for intellectual curiosity, have probably stimulated more scientific and technological research in the past quarter-century than in any comparable period in the world's history. At least the statistical studies showing the exponential growth of the number of scientists and scientific papers would seem to indicate some correlation with increased financial support from the government. This begs the

question of the quality of the scientific research, but with the lack of any firm indicators, we must assume that much of it was trivial, some of it was worthwhile, and a relatively small amount of it was truly significant—just as in the case of most scientific researches throughout history.

President Eisenhower in his talk seemed to link intellectual curiosity with blackboards, and government contracts with computers. But let us not confuse presidential rhetoric with facts. While it is true that government funds have been used to purchase computers for scientific research, it simply is not true that there are hundreds of new electronic computers for every old blackboard. And if there were, I should say that the situation deserves at least two hurrahs rather than dire forebodings. For the electronic computer is a powerful tool for the human intellect, an invaluable servant of human curiosity, enabling us to seek for the answers of questions which could scarcely have been asked before.

In any event, the blackboard remains; it is far from obsolete, and it still assists us in scientific-technological research. Ike was a great enough man to be forgiven his nostalgia for the past; the rest of us must live in and prepare others for tomorrow's world, and in that case we need the computer as well as the blackboard.

Science-Technology and the Military in American Society

With this reference to our late President's nostalgia for the past, we are ready to look back and see just where our study of the interactions between science-technology and warfare during the post-World War II era have carried us.

First, we have attempted to show that science and technology play a greater role in warfare than ever before in human history, and that a military technological revolution has accompanied the scientific and technological revolution of our time. The military commitment to science and technology is shown in the spending of the Defense Department for research and development, the increase in the number of scientists and engineers employed by the military itself or indirectly supported by defense expenditures, and DOD support of the education of scientists and engineers as representing a resource for national security.

Furthermore, defense expenditures for science and technology promise to continue into the foreseeable future. Even if the Vietnamese war were to end tomorrow, we have been told that defense

expenditures would not decline precipitously. Given the fact that military power in today's world rests increasingly upon scientific and technological capacity, we can expect military R&D expenditures to be maintained at a high level even if peace should break out. The level of support for science and technology in our military establishment is quantitative evidence of the way in which the military interacts with science and technology to a larger extent than ever before in our nation's or the world's history.

Although the military has become more closely tied to science-technology, military technology is becoming increasingly different from civilian technology. The specialized nature of military requirements, the sophisticated and complex weapons and instruments of contemporary scientific-technological warfare, and the varying production methods utilized for the new military technology have brought about a dichotomy between military and civilian science-technology which had not previously existed.

Paradoxically, just at the time when the products and processes of military science-technology are being distinguished from their civilian counterparts, there has been a rapprochement between the military and business, especially in defense-related industries, resulting in the formation of the "military-industrial complex." President Eisenhower's warning about the military's influence on political decision-making, especially when allied with powerful business interests, calls into question the distribution of power and the representation of interests in our nation's politics.

At the same time, the importance of scientists and technologists in the nation's defense effort has also brought them to the fore as a factor in political decision-making. It was once said that war is too important to be left to the generals, but increasingly war is being left to the scientists and engineers. Perhaps it is too important for them also. Although Don K. Price thinks that our democratic processes are sufficiently varied and that the centers of power within our country are adequately dispersed so that no one group can exercise a disproportionate degree of power over a long period,[75] the fact remains that a new estate—the Scientific Estate—has entered into the American political process. This has come about largely because of warfare's increasing dependence upon the scientific-technological parameters.

Because of the power of the scientific estate in political decision-making, the alienation of the scientists from the military poses a major problem, especially given the scientific character and foundations of

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[75] Don K. Price, *The Scientific Estate* (Cambridge, Mass., 1965), chaps. 3, 6, 8.

modern warfare. The fact that scientists and engineers can be wrong about military matters, as well as scientific and engineering matters, scarcely alters the magnitude of the problem. We must not forget that Vannevar Bush[76] predicted that tactical (that is, small) atomic weapons would not be possible, but this did not detract from his power in pushing for government support of science. All this proves is that scientists can be just as wrong as generals in their predictions regarding future weapon systems. The real danger to the country arises from the possibility that both the generals and the scientists will be wrong at the same time and about the same issues!

Entrance of scientists into the political arena not only brings about political problems,[77] but it also raises questions of possible military influence—really, interference—with research. Some academic scientists—and many of their students—feel that freedom of research has been endangered by collaboration with the military, especially by dependence upon government research contracts.

The threat of military interference with scientific freedom is not a myth. We already have one such example where the Army collided with its own scientists in a research organization set up to serve the Army's goals. In 1961, the relationship between the Army and the Operations Research Office (ORO) was severed amidst clouds of ill will on both sides. ORO scientists charged that the military had attempted to hamper their freedom of research and withheld funds in order to exert pressure; on its side, the Army claimed that the ORO was guilty of breaches of security. No matter which side was at fault, this episode reveals that the defense establishment might utilize its support of R&D to infringe upon scientific freedom. In every free society, including the scientific community, public discussion, high competence among those occupying positions of responsibility, an alertness to conflicts of interest, and the open airing of issues are essential for continued freedom.

Some measure of protection from possible military interference with science is afforded by the fact that other agencies of the federal government also support scientific and technological research. These other institutions, such as the National Science Foundation and the Atomic Energy Commission, were set up, not without struggle, under civilian and not military control. Although the Defense Department supports most R&D, the principle has become firmly rooted in American practice that civilian as well as military agencies can provide funds for broad areas of the scientific and technological spectrum. Further-

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[76] Vannevar Bush, *Modern Arms and Free Men* (New York, 1949).

[77] See Robert Gilpin, *American Scientists and Nuclear Weapons Policy* (Princeton, 1962).

more, it should be noted that these civilian agencies can "interfere" as much as the military. By granting or withholding funds, they can and do control the directions and nature of much scientific and technological research—even the purest of "pure" science.

At the same time it should be noted that some of the non-profit institutions for research and analysis, which had previously concerned themselves primarily with defense work, have begun to turn to civilian problems. Most notable in this respect is the RAND Corporation. Originally set up to do research for the Air Force, the RAND Corporation soon broadened that mission to include not only scientific and technological advice but also political and social analyses. It has recently offered the services of its task forces of scientists and technologists, including social scientists, for research on problems outside the area of national security. Indeed, a major concern of several defense-oriented research institutions these days is in the field of urban problems. By its prior and present support of these organizations, the Defense Department has built up America's capacity to deal with serious social problems besetting our society. So, just as scientists and engineers seem to have more say in military decision-making, they are now in a position, through their military support, to bring their R&D efforts to bear on problems outside the military sphere.

The above factors, I believe, are indicative of an institutional phenomenon which is new to American history, namely, the integration of the military into the matrix of American society. Apart from the early period of American history, when, under the influence of Jeffersonian concepts, the soldier and the citizen were regarded as one, and except during those brief periods of total mobilization for major wars, the military has stood apart from the mainstream of American life. In a business-oriented society, during most of the last half of the 19th century and the first half of the 20th century, the military felt alienated. At best, the defense establishment was regarded as a necessary evil, and Americans viewed the military with suspicion, especially when they saw the impact of militarism upon Germany, Japan, and other countries.

Most people would claim that the increased importance of the military in American life is due to our global political commitments and the threat and actuality of warfare since World War II. That is undoubtedly true, but I think that is only part of the reason. Beginning with the New Deal and increasingly since World War II, our government has entered into spheres of social welfare and communal responsibility which had not previously formed part of its domain. The military, as an important agency of our government, has participated in this trend. It has become a major factor in our educational institu-

tions and a prime force in industrial and economic life—and these largely because of the scientific and technological underpinnings of modern warfare. Indeed, the military has itself become a social instrument, designed to meet the goals of the nation, not only in terms of military security but also in terms of domestic problems. For example, the desegregation of the military was a prelude to the extension of civil rights in America. Similarly, with the outbreaks of violence in American cities, the military is increasingly called upon to maintain civil order. In brief, the military, once alienated from the mainstream of American society, has become a major current within that mainstream.

As part of the mainstream of American life, it is not surprising that the military meets with eddies of disturbance and resistance. While the military establishment tends to look askance at the criticism levelled at it, it should be remembered that much of the criticism would be irrelevant or non-existent if the military had not become integrated into America's social, economic, political, and cultural life.

Indeed, all three elements of our concern—science, technology, and warfare—form an integral part of modern American civilization. Our industrial civilization rests upon a high level of science and technology, which in turn are bound together as manifestations of that society, as both cause and effect, in a complex manner. The increasing importance of military institutions in modern American life thus depends in large part upon the fact that the military has become inextricably intertwined with science and technology. As these have become significant components of our national life and our national security, the military too has gained a major role in American society.

Commentary

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I have been deliberating for nearly two months, and through several drafts, as to what approach I should take in this commentary. I can assure you that the problem still eludes me somewhat. For one thing, I very much appreciate Professor Holley's presentation because it stands as testimony beyond his major study—*Ideas and Weapons*—that he has the courage to venture into areas of history too forbidding for most of us. For another, the options available in any consideration of the "action-reaction and interaction"—to borrow Dr. Kranzberg's telling phrase—of science, technology, and warfare after World War II are so great, that I felt like a practitioner of systems analysis, but without the sophisticated techniques and rigorous discipline to which the experts aspire. But after re-reading the papers of Professors Holley and Kranzberg, after re-thinking the ideas raised by the Symposium, and after giving second thoughts to the fact that I am a social and political historian thrust suddenly amidst this exceptional coterie of military historians, I decided to accept the invitation of Colonel Hurley to discuss the subject in broad terms. More specifically, I decided to focus on what I consider to be a very significant implication of Professor Holley's paper, and then to supplement it by a swift retreat to my own competence and research.

That implication, or message, as I read it, is that the alliance between science, technology, and warfare in the postwar period has never been so easily and effectively consummated as outward manifestations might suggest. It is not automatic. It takes place in the flux of history, subject on the one hand to the vast range of personal emotions—of hope, aspiration, prejudice, envy, fear, and uncertainty—and on the other to the unpredictable demands of external events, whether they be economic, political, social, cultural, or intellectual. More precisely, Professor Holley portrays one aspect of that relationship, which was marked by unfulfilled expectations, or in his own words, which began as a "wartime triumph" and "fell short." I can sympathize with the nostalgic disappointment which on occasions finds its way into his story, for operations research should seemingly have shared in the remarkable

growth and success which was characteristic of most other areas of the scientific-military alliance. It had made vital contributions during the war; it had earned the respect and dedication of military leaders; and it did answer to the needs of the Cold War. Yet Headquarters Operations Analysis "wandered for 20 years in the wilderness." At the heart of the explanation for this, as Professor Holley has told us, was the problem of organization and top management, of structure and leadership. But a significant part of the explanation also had to do with the human or personal: the fear on the part of a group of professionals about losing the respect of their colleagues; their suspicion about outside analysts; their inability to alter their standards and life style, or to define their *raison d'être* in a period of rapid change.

Operations analysis did not reach maximum effectiveness, therefore, primarily because of internal complications or inadequacies within the Air Force. Future studies, I suspect, will show that other postwar efforts have suffered from similar and equivalent internal limitations. But the military-scientific-technological relationship has also been a victim of powerful external pressures. My own research on the United States' importation of some 640 German scientists and engineers between 1945 and 1952 clearly shows the impact of politics, economics, and morality upon that relationship. The thrust to import the Germans, and thereby integrate an entire nation's technology into our own, was strong and forceful within the services. Yet historical trends, profound and for years unyielding, restricted the program. I would like to briefly note some of the major influences as a kind of complement to Professor Holley's theme.

In the spring of 1945 there was no policy in Washington to import enemy scientists. Indeed, such considerations as had been made were all intent on keeping them closely controlled in an occupied Germany. The State Department's Safehaven program aimed at preventing the travel of scientists from the Reich to other nations, especially to Latin America; and the Foreign Economic Administration's studies on how to keep Germany from ever again becoming a threat to the world, insisted that the scientists were needed for the reconstruction of their homeland and could make no conceivable contribution to our own weapons technology. Yet other forces were leading toward a different conclusion. The wartime respect accorded to Germany as the world's leading nation in science, the widespread apprehension about her "wonder weapons," and the immediate need for technological intelligence about weapons in use, created a compelling fascination about her accomplishments. Beginning in 1944 a host of technical intelligence teams descended on the continent; their interrogations and their findings led to requests for the immediate evacuation of several hundred personnel.

By July of 1945 the Joint Chiefs of Staff had acted favorably upon the requests through the establishment of Project Overcast, which provided for the temporary importation of as many as 350 scientists for a period of six months. Yet the end of the war in September destroyed the rationale behind their program—that the scientists could contribute to the war effort against Japan. The issue nonetheless remained alive. Some officers pushed for a permanent immigration plan to augment our weapons technology, inspired in no small measure by their excitement over the utilization of small clusters of Germans at Wright Field, Fort Bliss, and at a naval installation on Long Island. Other military personnel were concerned about the delicate problems involved in using former enemies, a sentiment that met almost unanimous favor within the scientific community.

The State-War-Navy Coordinating Committee, upon whom the responsibility for a decision rested, was unable to resolve the issue until February 1946. By then a new purpose had entered the minds of the policymakers. The intransigence of the Soviet Union, exemplified by Joseph Stalin's threatening speech early in the month, and George Kennan's famous telegram and the Canadian spy case later, led to the conclusion that the importation of the most outstanding Germans was necessary in order to deny their services to an ever more "potential enemy." In March the State-War-Navy Coordinating Committee established Project Paperclip, which would allow for the permanent utilization of as many as 1,000 scientists, who would hopefully become American citizens. The project had the support of the Secretary of State, James Byrnes; the Secretary of War, Robert Patterson; and the Secretary of the Navy, James Forrestal.

The military's seeming success in providing for the national security proved to be an illusion for the next six months. In the late spring of 1946, two historical trends interrupted the policy process. On the one hand, influential Congressmen were working to amend the immigration laws to exclude from citizenship any persons who had ever been a member of the Nazi Party. On the other, some members of the State Department were striving to implement the Safehaven program and keep all former Nazis out of the Western Hemisphere. Only the continued concern about the Soviet Union saved Project Paperclip from an early demise. In September 1946, Dean Acheson presented the program to the chief executive. President Truman quickly gave his approval.

Yet even the President's sanction and the certain arrival of the Cold War could not ensure the maximum effectiveness of the German scientists program. In early 1947 the Federation of American Scientists combined with numerous civil liberties groups in an effort to block the

effort. They failed, but the parsimony of Congress with respect to defense expenditures prevented the fulfillment of the high expectations. And policy entanglements continued. As late as 1952, the Pentagon was still pushing for new legislation to legalize citizenship for many of the German enemy personnel. Only when the Korean War gave the highest priority to weapons technology were the services able to relax from the long ordeal of policymaking. But the combination of traditional historical forces, politics, public opinion, and the misunderstandings and disagreements about the national purpose had been costly. The military were finally able to import only 65% of the 1,000 scientists authorized by the President.

Thus we have in Professor Holley's study of Operations Research, and in Project Paperclip, two instances of the postwar interaction of science, technology, and warfare wherein the performance was not equal to the promise. To borrow again from Dr. Kranzberg, operations analysis suffered qualitatively, Paperclip quantitatively. One might argue that the two cases were exceptional—that the experience of operations analysis stands in stark contrast to the overwhelming success of the broader science-warfare relationship, and that Paperclip was unique with respect to both time and purpose. I propose, instead, that many of the same influences which plagued them are still operative.

With due apologies for using the term relevance, I suggest that the military-scientific-technological arrangement cannot escape the current controversies over such issues as the ABM, the inquiry into the C–5A jet transport, the overrun on military contracts, the employment of minority groups on those contracts, and the increasing reference to the oversimplified epithet, military-industrial complex. The nation is very much divided, the decisions regarding priorities for the national interest are exceedingly complex, and the electorate of our democracy is still capricious. As the *Wall Street Journal* put it last week, "Now in 1969 the fight over a once routine authorization for military hardware promises to be titanic, one of the great congressional battles of the century." This may well be an overstatement; the scientific-warfare association will obviously continue, but insofar as values and funds affect it qualitatively and quantitatively, it is still not free from the dictates of the American experience.

In conclusion, I return to a comment of Professor Holley: "If one fact stands out above all others, it is the absence of historical analysis, self-conscious, introspective, analytical concern for the on-going OA organization and its processes." This same lack of analysis and concern has been true with respect to virtually all historical inquiry into the problems of technology and warfare after World War II. This Symposium is a promising exception.

Discussion

THE CHAIRMAN (Professor BERNARD BRODIE) : We have joining us three additional PhDs, one of whom is also a Colonel: Dr. John Fisher, who is currently Chief Scientist of the United States Air Force; and Colonel Francis Kane, who is presently a lecturer at UCLA on strategy, war, and revolution. On Dr. Emme I have before me four pages of biography, but I shall content myself with saying that he is now the historian of the National Aeronautics and Space Administration. I am going to turn the discussion over to the panel, but I am not going to give away free this last opportunity to make a few remarks of my own.

Someone has to play the devil's advocate and though it has been done here to some extent, I think it could usefully be done more. Most of the discussions have centered around the imperfections of the development of systems analysis and various forms thereof, and various speakers—I am thinking especially of Mr. Perry—have spoken very trenchantly upon that. But I feel there is another problem. I feel we have paid a high price for the amount of operations analysis or systems analysis that we have already achieved. It might be somewhat an exaggeration to say that I bow to no one in my reverence for systems analysis, but it is true that I have very deep respect for it, gained over fifteen years of close contact with it at the RAND Corporation. But we have also been witness to the fact that over seven of the past eight years the Defense Department was administered by a Secretary of Defense who seemed entranced with this particular technique, who is reputed to be brilliant, and who seems to have accepted the idea that the area of systems analysis is really coterminous with national strategy or national defense policy. That leaves some pretty important problems outside, such as political problems. I think Professor Kranzberg was a little optimistic when he talked briefly about the intrusion of political science into this area. I think its intrusion has been extremely limited.

What I am saying is essentially the following: most of the people whom McNamara gathered around him had so much prestige because of their special skill in this special area that they were very free in giving advice in various areas that had nothing to do with systems analysis. And their advice was often accepted. It is, I think, another example of the price we usually pay when a certain kind of competence becomes unduly prestigious. I am referring particularly to the results

in Vietnam. Inasmuch as Vietnam has absorbed only about 3% of our GNP, I would hold that it could not have mattered critically whether our efficiency in weaponry in that area were increased by a factor of two, or perhaps five, or reduced by as much. What mattered critically were other problems. I would submit also that Mr. Clark Clifford in his single year as Secretary of Defense did a greater service to the nation in causing our administration to reverse course on Vietnam than Mr. McNamara, brilliant as he was, did during his seven years.

I should now like to ask whether I can get concurrence on this, and I address the question first to Dr. Fisher.

Dr. JOHN C. FISHER (Air Force Chief Scientist) : I am not going to answer that question. The Chief Scientist's job description, which I read after I took the job, is to advise the Chief of Staff and the Air Force on all matters of research and development. In order to do this he is given a staff of one executive officer, a secretary, an office in the Pentagon, the equivalent rank of lieutenant general, and unlimited travel. I can have anything and do anything that I can do by myself. My job is to look to the future—way to the future—ten, fifteen, twenty years to the future—and help point the way for the Air Force as to what R&D we should do for the next decade, so that the decade after that we will be in the right place.

If you notice, all you gentlemen are looking this way, toward the lectern. You are all historians—you all look in the same direction, toward the past. I am looking in the opposite direction. We meet together here—the two groups of us—spanning all of the past and all of the future. In a sense I am a prophet.

I would like to join the past and the future together insofar as it is possible, and I am going to draw a little map on the blackboard to help me do it. I believe in computers, but I also believe in blackboards. The upper line is 100%, the lower line 0%, of the people in western society. The horizontal dimension is time, with "now" in the middle. Back here on the left is a couple of centuries ago, over there on the right is a couple of centuries in the future. At the present time (and don't believe my numbers too precisely) something like 5% of our population is engaged in technical work—research, development, engineering, that sort of thing. I am going to plot at the 5% level the percentage of our population engaged in technical activity. The number of men in western society working in this area doubles every 15 years. The percentage of the population doubles only about every 25 years, because the population is growing too. Since I always like projections that will end somewhere, I draw this one as a percentage of the population. That means that if I go back 25 years ago, only half of today's

percentage was engaged in technical work; and 25 years earlier it was only half that. The percentage of the population runs down like that in the past.

That means that in my lifetime as a technical person—I have been in this business for 30 years—about three-quarters or more of all the technical work done in the history of man has been done. I have been around while it happened. That's why we call this a revolution; and it's still going on. I know personally or have seen most of the great scientists and engineers of history. Most of them are still around. Not only is this true now, but this was true of men my age 50 years ago and of men my age 100 years ago. It's been that way for 200 years, that in any given technical man's lifetime most of the technical work up to then had been done.

Now my point is that this situation is not going to continue much longer. It has to come to an end. This is why. Let's look ahead 100 years. In 25 year periods, if we continue doubling our technical effort as a percentage of our population every 25 years, it will get up to 10%, 20% 40%, 80%, and within just over a century it would pass 100% of the population. It can't do that. So we have to believe that it will level off. Where will it level off? Actually we can see it beginning to level off now. It is leveling off because the amount of money it takes to do what we are doing is noticeable, and it is competing with other people's demands for funds. So, my guess is we are right now experiencing a bending-over point of an S-shaped curve, and the proportion of scientists and engineers will likely level off—or even more likely decline—as a percentage of the population.

After we have been leveled off for a long period of time things are going to be quite different. Suppose that I am making this speech 150 years from now. I might then say that in my lifetime, 5% or 10% of all of the work in science and technology in the history of the world has been done; and, as a matter of fact, I know one or two of the great scientists—not all of them, for most of them have been dead for years—but I know one or two of the great men. That means the revolution is over. The action was in the past. Furthermore those who are alive 150 years from now cannot expect to do a large portion of the technical work that has been accomplished in the history of mankind. If they are lucky they will add to it a little bit. That will be noticed by the sponsors of this kind of work, and they won't be putting up the dough any more. That's why I believe it is likely that this curve will turn over and go down. And we'll settle down to some small proportion of the population being experts in science and technology. They will be caretakers. They will be a priesthood. It will be their duty to spend

their lives trying to comprehend what went on back in the old days and explain it, keep it alive, and indoctrinate the new priests as they come along, so that this tradition will be kept going. I don't know at what fraction of the population it will level off, but perhaps the same as any other priesthood, a reasonably small proportion of the population of our society.

Now what consequences will this have for the subject at hand today?

First, it will mean that science and technology will again become decoupled from the military. Nothing much will be happening in science and technology any more, just as nothing much happened with saddles and the pike and the instruments of warfare in antiquity. They will become decoupled and running wars will again become an art. Of course, the tools the soldiers have to do it with will be fantastically complex compared to what they have now. The revolution is not over yet. We have another century to go, and a great deal is going to happen before it ends.

Second, who is going to keep the tradition alive? I claim it is the successors of you ladies and gentlemen. The historians of science and technology are not only going to have to keep alive the memory of the men who did it, and the organizations that did it; but also they are going to have to transmit the body of science and technology. The future of science and technology really belongs to you, not to the people who are at the moment carrying on the revolution. Their days are numbered. Certainly the future holds little for prophets. When we arrive in those times, we will know that the more distant future holds nothing new. Historians are going to reign and Chief Scientists are simply going to fade away.

Colonel FRANCIS X. KANE, USAF (Space and Missile Systems Organization, AFSC): Professor Brodie, I submit that I answered the question you posed about Vietnam implicitly in my article in *Fortune* in 1964. I said then, as I say now, that systems analysis is an art. It requires more than cost effectiveness. It requires more than ops analysis. It requires the inputs of political scientists and others to make the weighty decisions on strategy which must be made in the military sphere. Lacking those other inputs it was inevitable that our course of action would be insufficient.

Well, I have been a planner for 25 years. Planners are supposed to look ahead. I didn't look ahead when I accepted Colonel Hurley's invitation. I didn't realize I would be the man in the black hat when I got here. I have been at the center of the decisions that Professor

Holley talked about, when we weighed the fate of ops analysis in the Air Force, when we weighed the fate of systems analysis in the Air Force, when we weighed the impact of technological revolution. In fact, I guess, I am unique in that I have the only continuity of that whole process from 1946 until today. Therefore, I must disagree with history as recorded by the historian and say, "That ain't the way it was, Charlie." And I do that with great reluctance because my horoscope this morning said, "Don't be critical." And I do it with great reluctance for another reason. I draw heavily on his book, *Ideas and Weapons,* which I think is a landmark in the whole process of understanding relationships between technology and strategy. But my sources are different because I was on the scene.

Let me recount the way I saw the situation. In 1946, I was in the hospital at Walter Reed waiting to go to Georgetown to be part of the Jesuit plot. So they sent me to the Pentagon for a few months and there I got involved in the kind of statistical projections Professor Holley discussed in the demobilization period, and there I got to know the ops analysts of the Air Force. In 1949, after being at Georgetown eighteen months, I was put in War Plans where we did the first comprehensive systems analysis undertaken by the Air Force. Its very peculiar title indicated that we were just at the beginning of a revolution. It was called the Air Force Mobilization Plan. We were still laboring under the illusion that in the defense of Europe in the 1950s we could still have time to mobilize. So, we started off with that assumption. We turned to our friends in ops analysis to help us with those problems. We soon learned that they were, as Professor Holley said, worrying about aircraft problems and the way aircraft operate in tactical situations and operational situations, whereas our concern was entirely different.

We were trying to do things which are normal in the analysis we do every day in the Air Force, that is try to introduce new weapon systems. We had a top secret annex and we hardly breathed the words Navaho, Atlas, although we had to say when they would come into being, what they would cost, and what aircraft we would give up as a consequence. We also had to show how we would lose aircraft as we tried to attack Russia—deliver weapons on Soviet cities in the face of their defenses. Later those analyses were given an esoteric term, "draw-down curves." That was a key word in the early 1950s when some of our colleagues from RAND arrived on the scene. We did "draw-down curves" that startled the Air Force generals because they assumed as in World War II that once war started, numbers of units, numbers of people, numbers of aircraft would continue to rise. So we did a lot of innovative work trying to understand the new situation we faced, finally coming to a realization that the World War II experience had

limited relevance for the future of the Air Force in the defense of this country.

Now at the same time we performed another great service. We helped RAND get started. The ops analysts apparently don't like this innovation. But as we were doing these various projections of war in the 1950s we had several overseers come from the embryonic RAND office and spend weeks with us. Later they went back and briefed General Landon on the wonderful innovations they had discovered and they were proposing he adopt. So he called us all in to hear the briefing from our colleagues in RAND, and they told us what we had been telling them for several weeks. As a novelist said, "The briefing was followed by an embarrassed silence." But in any event, since then there have been very close ties as we built up the really comprehensive interplay of all the elements of strategy that must be analyzed to make the kind of decisions which we have been making and must continue to make in the future. Now in that whole period we drew on ops analysts as individuals. We never drew on ops analysis as an organization.

The Air Force sent me off to France for three years, and I spent a year at Maxwell writing my dissertation. I came back to the Pentagon to find that the ops analysts were still worrying about aircraft problems, and we were trying to solve missle and space problems. When I returned to the Pentagon the next time in 1964 to try to start the office called Studies and Analysis there still wasn't a single analyst who was worrying about space operations, although we had trained by that time hundreds of Air Force officers at Maxwell to understand the fundamentals of space operation.

Now I had hoped when I read Professor Holley's paper that he was finally going to tell me what was in Paul Hower's mind when he kept ops analysis out of Studies and Analysis, because I was the one at the blackboard drawing wiring diagrams and trying to put people from ops analysis into Studies and Analysis. We didn't get involved in the continuation of doctrine, and we didn't get involved in the traditions of ops analysis. We worried about the fact that certain ops analysts were GS 18s and therefore couldn't work for Majors, and other ops analysts were GS 19s and couldn't work for Colonels. And so at 10:00 or 11:00 each night we would go home with nothing resolved. So I think, Professor Holley, you should turn around your moral and say it's a case of pure Parkinsonism. A group of people filled the need at one time, but they became obsessed with that problem and failed to keep in touch with what was really going on in the world of strategy. They became so intent on their own operational problems, their own bureaucratic problems, that they restricted themselves to what they set out to do and that was to

worry about aircraft tactics in certain situations. Since we have had few occasions to worry about aircraft tactics—World War II, Korea, and Vietnam—their usefulness returns to the scene only when those kinds of problems are addressed. That is why RAND filled the void that ops analysis didn't fill. That is why other companies—STL—filled the void which RAND didn't fill. Now we have hundreds of people doing ops analysis of space operations and we have very few people at RAND who do those problems, and we draw on them as individuals just as they used to draw on ops analysts as individuals. Some of my closest friends are people like Fred Nyland, Russ Shaver, Ted Parker, Don Emerson, and others who work with us on specific problems that we have in identifying decisions to be made on acquiring future systems.

Having been at the heart of the problem of using analysis for decision making and having been one of the participants in the revolution and still pushing the revolution in technology—as a matter of fact I have forty projects currently underway, all for future systems or technology—I conclude that the revolution is far from over and that our impact on the civilian side of our economy is going to be greater than ever. For example, we are trying to invent a navigation satellite system which will completely revolutionize air traffic control of the civil fleet and will probably be used by all the boat owners in Santa Monica harbor. Those indirect applications are going to be as much leaven to the civil sector as have been the direct transfer of technologies into building TVs and transistors.

I would like to conclude then with some further observations about the whole process. As I see it, we must remember that technology leads strategy. That's a fundamental concern of us who are in the technology side, because we have not only to make the changes, we have to understand where they are going. We don't depend on an organization, Professor Holley, to keep up our doctrine. We depend on ourselves, the individuals who are making the changes, to see what the impact is. We do special studies such as one we call Strat-70. What is the impact of future technology on strategy? What is the impact of future space technology on tactical operations? We look at those problems to understand the changes which are required in future operations. And they come not from an organization sitting off to one side, but from people in the main stream of the changes they are creating. Technology leads strategy and therefore puts a special burden on the developers of strategy.

Secondly, technology is additive. Like Professor Brodie, I started off with the National Guard in the horse-drawn artillery, except I did it by a subterfuge. I was a Boy Scout. I loved horses and I wasn't old enough,

so the Guard took on a troop of Boy Scouts to help us learn to ride. The Air Force still has horses. Technology is additive. We still keep the weapons we had before. The foot soldier still uses the pike, but he calls it a bayonet and so on. We have to look at the choices available from the technologies of the past as well as those of the future.

And here I would like to close on my negative note, but also a hopeful one, because the future is still undetermined. The main thrust of the past several years has been a denial of the use of history. We have had an anti-historical attitude at the decision making levels of the Defense Department. Everytime we try to derive future needs from past problems, we are told, "Don't look at the past earlier than," say, "the invention of the ICBM. You cannot equate the strategies and problems of the past with those of the future." The historian has to prove that that attitude is completely unsound. We must derive our future from the past. There is no other way to do it.

I have about 275 people working for me, of which 150 do analysis, and of which 50 do ops analysis. Every six months Professor Quade from RAND comes over and runs a course on analysis for all the new officers in SAMSO. We always have the same debate. I say, "What comes after analysis?" All the innovations have been made by people under 32, and the people in the audience are all under 32, so I pose the question to them, "What comes after analysis?" Professor Quade always says, "More analysis." Well, I believe something more than analysis lies in the future and I believe the heart of it is to take a step called building a technological strategy, which tries to integrate all the past history, the lessons we have learned from the past, the potentials of technology for the future, but more important the leaven of policy which can come only from the political side of the family.

Thank you.

The Chairman: Thank you, Colonel Kane. I find it positively charming to know that the Air Force still has horses. Colonel Kane didn't tell us the function of them. I thought commercial fertilizers were cheap, abundant, and very effective.

Now, Dr. Emme should not be denied the opportunity to say a few words.

Dr. Eugene M. Emme (National Aeronautics and Space Administration) : Commenting on this morning's papers on recent history, I submit, we should recall at once what Lynn White suggested in the opening session: There are dangers of over-specializing on the concerns of science, technology, and warfare without looking at the total fabric of things and people, machines and men, and their ideas and institutions in their

temporal environment. Bob Perry's quotation on the search for knowledge for its own sake as very much like doing things just for the hell of it, appears precisely why I find myself trying to summarize this entire symposium in a couple of minutes.

Without delaying our bodily needs of lunch, I am becoming very prejudiced about intellectual exercises called "history" which are not. There are the case-study analyses like Project Hindsight, to which Professor Kranzberg referred. Many of these take Monday morning "historical lessons" and attempt to trace backwards to find why Saturday's game with Army was lost during practice the week before, or even spring training. Some of these analyses are so "valid" that it was not even necessary to play the game itself to find out the actual score. These and other "historical lesson" examples are what I call "C.P. Snow-jobs" based on mere two-dimensional or first-order relationships, and ignoring many other factors and the processing of events in time. Modeling is now to be the pastime of historians?

The point I am trying to make is that inductive modeling was once an educational device of U. S. Army officers at Fort Leavenworth, where they used sand boxes and tin soldiers and horses on tactical problems. If you want to get into more comprehensive system analysis, you could also go back into military history for interesting origins of dealing with complex engineering of technology with military forces. In 1807, William Congreve, in "The Rocket System," described the introduction of gunpowder rockets to military forces. Even Robert McNamara would have liked Congreve's detailed systems analysis for a proposed weapon system, which not only specified "modes" of operation for all conceivable situations, but also included relative costs as regards forces other than the Rocket Corps. Congreve was persuasive enough to get the Rocket Corps established in the British Army. With success rockets became standard in all European armies and are mentioned in "the rocket's red glare" of 1814 in our national anthem.

So far in the commentary this morning we did make some progress by getting away from the "chicken-and-egg" theory of history, which is fine; and then we got into a "push-pull" thesis of history. I was most attracted, however, by Dr. Lasby's "flux of history" concept, which is at least three-dimensional and includes evolution of time in my lexicon.

I would like to plead that we have had less solid history submitted in this session than in any of the previous sessions. Perhaps I am blessed as a historian in being in a civilian space agency. Clearly the rationale for space exploration just for the hell of it is most persuasive for curious scientists as well as the adventurous desiring to explore because it is there. Based on technology, much derived from military missilery, space

exploration was ignored pre-Sputnik because it had no single logical military consequence. Hopefully this July, with the help of military technology and some of the military R&D managers, the first men will set foot upon the surface of the moon. This will all be part of the revolutionary era in which we now live, when all of our institutions are not only very large organizations but also are being impacted by dynamic changes in science and technology. While we may be having a new renaissance similar to the 15th, 16th, and 17th centuries, it is not news that the American people traditionally detest war, taxes, and government. All this is as much related to general American history as it is to science, technology, and warfare itself.

We have not had a major war since the nuclear weapons for destroying ourselves have existed and, of course, we have been constrained to engage in obsolete types of total warfare on a limited scale, and even enlist new technology. I still have a $5 wager to collect from a former SAC officer who, in 1955, bet that an iron bomb would never be dropped from a B–52. The B–52 was designed for massive retaliation concepts of national security or "waging peace." And yet, the B–52 dropping iron bombs may be our most effective weapon system in the guerrilla action in Viet Nam. Remember now my plea for solid history in its own vintage. We do not yet have a history of the Atlas, the Titan, or the Navaho missiles. In NASA, we are blessed by having less classified information for our histories. It was possible to publish a fairly solid history of Project Mercury despite the fact that the Air Force has not yet been able to publish a history of the Atlas booster, which made Mercury possible.

As historians get into the contemporary period it is very important not to get swept into the facile theories for explanation. There is a real need for the contemporary historian dealing with the people still alive and with strong memories to get to the core of why and how things happened as they did. We need to get close to the heart of the story on the important aspects of the history of science, technology, and warfare, if for nothing less than helping future historians avoid perpetuating legends and writing fiction.

One good example I should give here as the NASA historian. Looking at the national space program, beyond NASA and the military aspect, some of the basic facts are yet missing. President Lyndon Johnson in July 1967, in Tennessee, said that the military space program of the United States, not NASA's now, had achieved certain accomplishments in the waging of peace that had justified the cost of the entire American space effort, and including the landing of Americans on the moon. These are the kind of facts that have to be dug out, sorted, and put into

relevant context as the historian goes about his age-old job of describing how it really was in the 1960s. If we attempt to essay it as we would like to think it was, or to help us solve tomorrow's problems, or even to understand today, it will not be documented as it was.

Thank you for being so patient.

The Chairman: With that we can adjourn the session and proceed to lunch. Thank you very much.

THE 11th HARMON MEMORIAL LECTURE IN MILITARY HISTORY

Each year since 1959 the United States Air Force Academy has sponsored the "Harmon Memorial Lecture Series" in memory of Lieutenant General Hubert R. Harmon, the first Superintendent and "Father" of the Academy.

A committee of nationally-known civilian historians and Academy representatives selects an outstanding military historian to be the lecturer, who is then invited to present an original lecture on a subject of his choice within the field. This is the 11th in the series sponsored by the Department of History.

THE WAR OF IDEAS;
THE UNITED STATES NAVY, 1870-1890

Elting E. Morison

Yale University

Cadet Commander Martin, Cadet Roselle, and the members of the Cadet Wing: It is of course an honor for me to be asked to be one of the members of the distinguished list of Harmon Lecturers. It is also an honor for me to be here in Arnold Hall. In fact, if it had not been for General Arnold perhaps none of us would be here. He was thought by some to be innocent and simple. This was a deception. He was an extremely skilled negotiator and dedicated to whatever purpose he had in mind. The purpose he had in mind above all others was a separate Air Force and he contributed markedly to the attainment of that objective. Hence, you are here; hence, I am here. It shows you what one man can do even in a complex and large system like an Air Force.

I am here under certain handicaps. The previous speakers on this program were real military historians. They were old pros—I am not. I have done some work in naval history in a period now long gone, and I have spent most of my time in that period thinking about the Navy as a society rather than an armed force, trying to find out in a kind of sociological way what happens in a highly articulated, neatly organized, closed society. So I appear with some diffidence following these others who, as I say, have been old pros. I also have a feeling of diffidence or handicap in other ways. I am told, for example, that some of you think of this room as a master bedroom—that you tend to go to sleep here. Then I have a third diffidence. My subject is largely the Navy and I have been told over and over again that this is not a subject which has first claim to your interest or affections.

I have, I hope, some redeeming features. The Navy that I am going to talk about is the Navy from 1870 to 1890, a period in which the Navy in fact did not look so good. You can take some superior satisfaction in that. Indeed, I do not intend to talk much about the Navy. I want to talk about another subject (and the Navy will give me an opportunity to do so) which I would call "The Care and Feeding of Ideas."

It cannot have escaped your notice that anyone who lives in this society today, whether in an armed force or outside of it, lives in an environment based in large part upon scientific understanding and engineering applications, and in order to thread our way through that complicated, densely intellectual environment, we must all master certain kinds of information and master certain ways of dealing with ideas. So I thought it would be more interesting to spend some time tonight talking about, as I say, "The Care and Feeding of Ideas," or the dangers of having too few ideas on the one hand, or on the other, the dangers of having too many.

I will start this investigation with the Navy of the period that I was billed to talk about, from 1870 to 1890. For much of the period that I will be concerned with there was little science, less technology, little invention, and fewer ideas. I think the quickest way for me to give you some sense of what that environment was like, what an armed force was like a hundred years ago, is simply to tell you a few stories or anecdotes. These will of course distort the meaning of the whole somewhat and I am aware of that, but I am anxious to give you a general feeling for what the world of the United States Navy in those years was about. We can correct some of the distortions later.

First of all I would like to talk about David Dixon Porter, one of the most celebrated naval officers who ever lived and the most effective commander in the Civil War. In the year 1886 he appeared before a committee of Congress to argue with all of the force at his disposal for keeping full sail on warships. This was 80 years after the *Claremont,* Fulton's steamship, had begun her regular duty between Albany and New York. It was about 45 years after the first merchant vessel had crossed the Atlantic under steam. Yet, the Admiral of the Navy approached the Congress of the United States to plead with all his force to retain full sail power on the naval vessels of the United States.[1]

A second brief anecdote deals with ship design. It occurred along about 1885 to some members of the Navy that they needed a new kind of ship, but they were puzzled by how to proceed because they had been building vessels out of wood (in a way that I will come to later) but they knew they had to try something new, and they had no one available to help them. So they told one officer to go about the shipyards of Europe and buy the plan of a useful warship for the United States Navy. He was obviously an indefatigable officer. He came up not with one plan for one ship but with four different plans for various parts of one ship, which he had culled from various shipyards. The resulting vessel was a

[1] Harold and Margaret Sprout, *The Rise of American Naval Power* (Princeton, 1939), p. 195.

composite of plans he had picked up from one British warship, two Italian warships, and one Chilean warship. She sailed for about 5 years, but she never sailed very well. This was in 1885.[2]

We come then to the question of energy within the military society. Target practice would be a good place to begin. There was a regulation that each ship should have a target practice every quarter—every 3 months. Now this was a distressing duty for many ships. It dirtied the vessel. You had to clean it up afterwards and you never had any great confidence that you were learning how to shoot anyway, because you only shot once every 3 months and you shot at small moving targets which you rarely hit. In fact one article in the *Army-Navy Journal* said, "It was a brilliant display of gunnery. All the targets were left untouched but it was a brilliant display." One resorted in this matter to remarkable methods of circumventing the regulations.

The most remarkable and ingenious circumvention was attributed to an officer who, finding that he was a little late and could not order up the target practice on time, had his men throw all the ammunition for the quarter overside and then took out the forms and filled in a fictitious set of target reports. Then, his conscience overcoming him, so as not to send in a fake report, he tore it up into small pieces, put the small pieces of the target report into a small box, put two cockroaches into the box, nailed up the box, and sent it off to the Department, the hope being that it would be felt that the cockroaches had eaten the target practice reports on the way.

We come next to another aspect of our problem. When the Navy began to build ships of its own, not having much expertise, it had some trials and experiments. They thought that one very interesting thing to do was to try to mount as many guns, to get as great a weight of metal as possible, on a small platform by doing what was called superimposing the turrets. You mounted the turrets for the 8-inch guns, which were about as large as they were building in 1890, and mounted on top of them the turrets for 5-inch guns. This was done to get a maximum amount of gun power in a small space. They neglected to take into account two things which became very apparent in the course of the first practice. One was that the turrets were arranged to swivel or turn on the same turning circle at the same time, but the correction for the rifling and wind velocity and everything else for the 5-inch guns was different from the 8-inch guns, so you never could train both sets of guns at the same time on the target. Also, they used the same ammunition hoist, and there was room for only one ammunition bag at a time, so only one gun

[2] Frank M. Bennett, *The Steam Navy of the United States* (Pittsburg, 1896), pp. 789ff.

could be kept going at a time; so the whole expensive contrivance, which was looked upon as a miracle of imagination, simply complicated the gunnery task enormously.[3]

Now I hope that, by these short little anecdotes, I have given you some feeling for the general state of the professional body of seamen at that time. There is, however, always in an armed force (you will find out soon if you have not already) the civilian side of the thing, notably the Secretary and his assistants. They are looked upon by civilians as the source of the most refreshing inputs into the military, who may get stale if they get sunk in their own juice. It is felt that civilians constantly bring in new ideas from the outside. In the middle of the period I am talking about, there was a Secretary from Indiana named Thompson. He had just been appointed. Indiana is an inland state. He went on his first inspection tour. He went aboard a ship. He looked down a hatch and was heard to exclaim in surprise, "Why, the damn thing's hollow!" [4]

Now these anecdotes give some distortion, but not much, about the general intellectual level of the Navy at that time. I would like to say one or two more things in general about the state of the Navy so that when we come to talk about ideas, you will have some feeling for it. Consider ships in the era 1870 to 1890. In general they were still built more often of wood than of metal, and they still were more often powered with full sail power than with effective steam power.

Let us take the work of the seamen and the sailors on a cruise. They stood watches, they shot the sun at noon, they kept watch, quarters, and station bills up to date. Standing watches was about all there was to do. It was what seamen had done when at sea for 300 or 400 years—a set of routines, arbitrary, clearly defined. They had a role to play. If you were at sea for as long as they were—frequent cruises of 3 to 4 to 5 months—it was necessary, having a ship's company that did not have too much to do, to have a set of rather arbitrary routines that held the whole society together and that in fact held the watch officer (who was a junior officer) or the senior officer himself together; but it was not a very imaginative or changing situation.

Consider ordnance. There were still a lot of smoothbores on the ships, of low power and little accuracy. As far as tactics were concerned, there were still people in 1890 who argued seriously that boarding and ramming were the major ways to engage in a sea fight. The great and

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[3] Elting E. Morison, *Admiral Sims and the Modern American Navy* (Boston, 1942), pp. 87ff.

[4] Sprout, *Rise of American Naval Power*, Chaper 11 gives an excellent summary of the period under review here.

fundamental wisdom about tactics was still Nelson's great dictum, "No officer can go very far wrong who lays his ship alongside an enemy."

In strategy the highest thought was that you existed to protect the coastline. You went out on a station if there was war and waited for the enemy to come to you. You then went close to her and at very short ranges either boarded or rammed or poured broadsides into her.

In all, nobody really quite knew why there was a Navy at this period. The definition of what a Navy was supposed to do and how it was supposed to do it was not clear. There was no naval doctrine. There were no strategic ideas and there were very few tactical rules except the rules of thumb. The result was a series of wooden ships mostly under sail (I am talking about most of this period from 1870 to 1890 at least) that went on individual missions following patterns of sailing that were devised shortly after the War of 1812. The mission was the suppression of the pirates in the Mediterranean, the prevention of the slave trade from Africa to this country, or showing the flag in alien ports. But in the last third of the nineteenth century, the pirates had disappeared from the scene, and the slave trade was over.

Naval society was run by faith and by habit. It had really no ideas at all. It never changed at all during this period and it was an exceedingly stable and pleasant life for many people. It was not, however, as though the seamen were in Eden before the serpent. In fact officers had had a taste of the fruit of the tree of knowledge. They did know much more at this time than their actions suggested. They had been through a civil war a very short time before, and in the course of that conflict they had learned that steam was infinitely superior to sail. They had learned that iron was infinitely superior to wood. They had learned that rifles were infinitely superior to smoothbores. They had learned that a blockade was infinitely superior to coast defense by isolated ships. They had, in fact, learned all the things they were turning their backs on. In the course of the Civil War two ships had been built that were twenty-five years ahead of their time. Fifty years after that, at the very turn of the century, a great naval designer said those two vessels were the greatest men-of-war that had ever been built. They had speeds that were not equalled for a quarter of a century. They had sea-keeping qualities that were not equalled for thirty years. They had maneuverability and fire power. They lasted exactly two years after the Civil War, when one was made a Navy receiving ship and the other was sold into the merchant marine.[5]

The Navy had the instruments, they had the demonstration that all

[5] Bennett, *Steam Navy,* Chapter 29 gives the fullest account of these ships.

of the things they had learned in the Civil War might make a brand new and effective and exciting Navy. Yet they systematically destroyed the weapons and turned their backs on the ideas. All the new-fangled stuff was turned back, and in order to assure that they would not have to deal with these complicated new systems and thoughts, the men who had been at the bottom of them, who were technical men, engineers and naval constructors, were either demoted or were put into stations or into positions or into areas of the Navy where they could do no harm by having new ideas. So they returned to paradise in 1865, which was the condition of things before the Civil War, and they could maintain this posture for several very interesting reasons.

First, there was peace and it was a real peace of a kind that we do not understand now. They had no view of a war ever happening again. Second there was no system such as what we now call the military-industrial complex. Steel had to be bought abroad. There was no effective steel company in this country right after the war. Ship designs had to be bought abroad. We did not have, once you got rid of the original engineers, anyone with enough know-how in the system. Third, there was Congress, as there always is; and congressmen were devoted to the idea of coastal defense so that they could tell their constituents that Charleston or Portsmouth or Boston would be protected by these single ships. This was a great comfort to people who lived there. Finally, there was (and I think this is one of the fundamental things), there was abroad in the land or in the Navy, no real intellectual notion of how to use the Navy, what it was for, or how to go about doing anything except sailing in these antique patterns. So back you went to look for the pirates who were not there, to repress the slave trade that did not exist, and to show the flag.

Now it sounds as though nothing was happening. In fact new ideas were floating about in this bloodstream, mostly among the younger officers. There was a man named Fiske who came up with a brand new range finder with a telescopic sight that he showed proudly to the captain of his vessel. The captain was a celebrated naval officer, "Fighting Bob" Evans. He took one look at it and tossed it overside on the grounds it was useless in the present situation. Then there was a man who recommended that armor plate be used, and for years he came up against the resistance of naval officers who felt that wooden ships were more effective. There was a man named Homer Poundstone who developed a new design called the all big gun battleship that fifteen years later became the major capital ship of Britain. There was a man named Sims in gunnery who devised all kinds of new ways of shooting; these, too, were sat on.

The reason for this was, as I say, that there was an interest in retaining a system which had been satisfactory to grow up in, and live in, and which did not seem to need to be changed; there was no understanding of why one should change. Finally, there was no way within the system to make all these things fit together. Someone developed a new range finder. What use was it if you were going to fight by ramming and broadside at close range? It could not necessarily lead to telescopic sights that would provide, after the range finder, a better bead on the enemy. These were isolated ideas that never fitted together because there was no general theory or system into which they could fit. I can give you an example.

Long ago in Athens a man named Hero invented a steam engine, a pretty good little model that actually worked. It was never used at all and dropped out of sight for centuries because there was no way to hook it up to anything. It could not do work with anything, it was an isolated idea; and it faded. This is very much the situation with the telescopic sight, with the range finder, with the new system of gunnery that could have been put together. There was no way for the society which had no use for ideas in general to make any use of these specific notions.

And then finally in 1890 an event happened that I think was as important as all of the other things that were helping gradually to move the Navy into a more modern place. Alfred Thayer Mahan wrote a book on the influence of sea power on history, and in the course of it he defined what use a navy could be. It could command the sea, and the way in which it could be used to command the sea was by general fleet actions, far from the coast, with fleets in being, fighting each other in the middle of the ocean. This defined for the first time, really, very clearly for officers and for people who thought about it, whether they were politicians or citizens, what a navy in fact could do, and how it could do it. Very shortly after this all of the random ideas that had been floating around in the society, ideas that had been thought of as products of rebels, of stormy petrels, of isolated men working alone, all these ideas found homes within a system—Mahan's—in which they interacted so that you could begin to build a technical system within which the Navy could operate effectively and understand why it was operating. It was not until a great, ruling, general idea came into effect that ideas in general began to work within the naval body. The Navy had been an entity—it had held itself together most effectively up to this time as a society, but mostly through habit. In about 1890 the force of habit began to be supplanted by a theory.

Now both habit and theory give pattern and structure to a society, but the one, habit, provides a rigid, resistant, impenetrable scheme for

going on exactly as you have, whereas the other, a theoretical structure, provides a pattern and a means for assimilating ideas that can relate to each other, that can change and move and grow. Now in all military establishments, as you well know, there is a certain amount of routine, and there is a certain amount of loyalty and devotion to routine. It is simply that in the Navy of the period I was talking about the devotion was too great and unqualified. I think any armed force can run, as any society can run, the risk of proceeding by habit and faith and devotion to certain primitive schemes until it runs out of energy and steam. As long as you are existing within a theoretical structure—a body of ideas—you have a chance to grow and survive. Now that is the first part of what I wanted to talk about—what happens to a society when it loses its interest in ideas and falls back on familiar patterns and ancient loyalties, however noble and however splendid a past they may have had.

I want now to speak about the second part. We will leave the Navy. The first part was the possibility of having too few ideas in a community. The second part is the possible danger of having too many ideas in a community. Today we are 180 degrees from where the Navy was in the previous century. The difference is as from night to day. We have a system going for us of pumping new ideas and devices into the whole society, although I am speaking at the moment just about an armed force. That system has its base in fundamental science, which is still conducted in the society mostly by universities, and in engineering applications that are still conducted mostly in industries and in places like the Bell Laboratories, and within the research and development agencies of the armed forces. You have as a result of this system of interaction between general and fundamental ideas and specific applications, a system that has markedly cut down, for one thing, the time from the moment you have an idea to its application.

Poor old Bradley Fiske, when he had the idea of a range finder, had to spend about fifteen years before he could get anybody to listen to him and had to take about five years more to make a good one. Today such is the system, it seems to me, that the lag between the first fundamental notion and the application is reduced, by the nature of the system I have mentioned, to a minimum. I could describe at great length, if you wanted me to, the nature of this process for systematically producing and developing new ideas. I can give you some feeling for the results of it very quickly.

I was in Pearl Harbor on a destroyer in January of this year, and I had not seen a destroyer in about eighteen years. The number of things on that vessel that I had never seen before, and the number of new things one had to learn to make use of those new things, had totally

changed the routines of a man at sea in a destroyer within the course of eighteen years, and in large part had changed the purpose, or the mission, of the particular vessel. We have got a thing, as I say, going that pumps in new notions so rapidly that we can in fact change large sections of our society in a very short time.

There is another thing I want to say about this system besides the way it has collapsed the time lag between the fundamental idea and the application. Remember it took literally centuries to go from the steam engine to its useful application. The normal course up to 1890 of an application of an idea after its fundamental, first thought was probably a hundred years, and now we have reduced it to, in some cases, a term of months. That is the first thing about the system that we have devised.

The second thing is what I would call the predictive characteristic in the system as we have built it; you can make an extrapolation from what you know you can do to what you think you may need in just a few years. Fiske, after all, when he had his range finder or his telescopic sight, had no idea of the system he was working in, so he had no idea of what uses to which it could be put, what organized system he could put it into, or what prediction he could make about where he would go from there. Today, however, all science in a way is a means of predicting what you can do. We now have in the scientific and technical way a method of saying that from this stage of the game it is only about ten years or five years or three months before we can proceed to the next stage.

I have two worries about the meaning of this extremely powerful system of ideas and mechanisms that we have put into the world. The first is, as with the destroyer, if we get to the point of thoughtlessly introducing too rapidly too many changes into an armed force, the structure that existed—the structure that the men in the last part of the nineteenth century wanted to preserve and protect because their very lives depended on it—might disintegrate under the load of new ideas and machines. Anybody in an armed force lives by a certain dedication to routines and loyalties and procedures inherited from the past. If you swamp those too rapidly—those old structures and routines—with a series of new findings that alter the way the men in the armed forces live, it may be too difficult for them to survive effectively in a very rapidly changing system. Indeed, they may in many ways find that things that they have done before are no longer possible to do at all, and they may have to find some new way of ordering their life as an armed force. So it would worry me some that unless we find ways of selecting and controlling the load that we put on an armed force, whether Army, Navy, or Air Force, we may put too great a social and emotional burden on the men in it to accommodate to rapid change.

I have a second worry as it relates to armed forces, one that is more complicated and one that I hope I can be clear about. It has to do with Clausewitz's statement that "War is a continuation of policy by other means." It is in our society an accepted belief that policy controls the use of arms—that arms exist to support a policy and that that policy is determined by the civilian branch of the government and therefore in a representative form of government by the civilians themselves. What I have wondered about is that with this capacity to generate new ideas rapidly, to predict in advance the long-range technical needs of an armed force, whether, given these possibilities, we will not all of us—civilians and soldiers and politicians alike—come to concentrate much too simply on the *means* available to us rather than the ends to which those means are put. In other words, I worry now and then that by concentrating upon the means of applying force, we may in some subtle way distort the making of policy in any *other* terms. We may lose sight of alternative policies that we otherwise might take into account, that might enable us to avoid the tragedy of war at all. We may tend to lose our sense that there are policies of various grades and sizes, policies that *various* kinds of power—not just military force—can be used to support.

Now, thus far I have spoken only of the armed forces, but I said to you earlier that my interest in them historically has been too look at them, to try to think my way through into problems that are more obviously part of the whole society but less easy to think about because most societies are more loosely structured, less articulated than armed forces, so you cannot see the effect so clearly. I think that what I have been speaking about is the possibility of overloading the structure of an armed force with new ideas and the possibility of getting so concerned with those new ideas that you lose sight of why you are developing them and what you want to use them for. This is not a problem for the military alone. It is a problem that we must all face together.

I think that the developments in biology which have given us a much fuller sense of what makes human personality what it is, what it might be, and how it might be changed, the developments in all areas of life that science can throw light on—and that is most of them—have given us a complicated system for introducing new ideas and new ways of dealing with things into the whole of society so that we may very well overload the existing classical structures. Clearly we have overloaded the cities. They cannot handle their problems. Clearly in some ways we have overloaded governments of all kinds. Clearly in recent days we have overloaded the classic structure of the universities. These are all symptoms, it seems to me, of the decay of institutions that have been overloaded by new inputs mostly from science and technology.

So if I worry about what happens to an Air Force as a result of new missile developments, I worry also about what happens to all of us, what happens to cities, universities, and organized governments of one kind or another, and our established habits and conventions. I think that what we all have to begin to think about much more clearly than we have is the question of what ends we want these means to serve. I think it means the development of new kinds of institutions and new kinds of criteria for judging, so that we can set up a restraining context–organized schemes like Mahan's theory that will enable us to control the extraordinary energies and applications that we have power over, in such a way that they will serve man and society most effectively.

I think this calls for the most urgent and concerned and dedicated cooperation among the scientists, the engineers, the social scientists, and the humanities, and any other elements in the society that have a concern for it, whether in industry or in armed forces or whatever. One of the reasons that I wanted to come tonight, and one of the reasons that I admire the Air Force, is that you seem sufficiently aware at the Academy of the importance of getting this cooperative venture going when all of us can begin to think about the development of new institutions, the invention of new kinds of conventions, and the creation of new kinds of cultures to enable us to hold in check the forces that we have let loose within a context that will serve us effectively.

To have historians join you in thinking about this and take two days doing it, and to have you join historians is at least a beginning, I think, in the kind of joint concern that we all have got to have if we are going to keep the show on the road, whether it is the Air Force or the Navy or the United States or the world as a whole.

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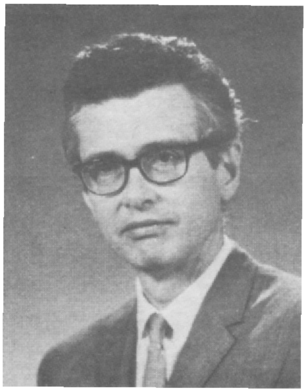

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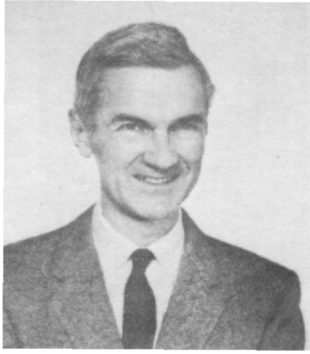

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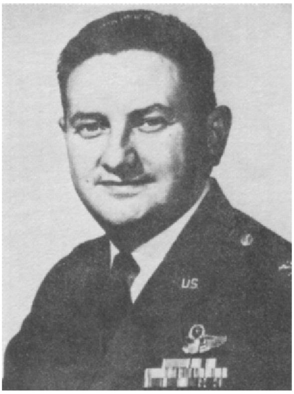

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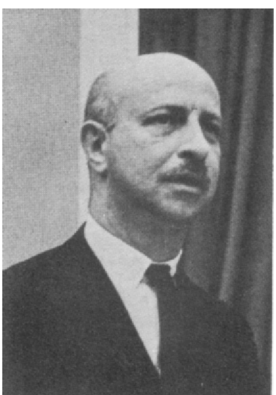

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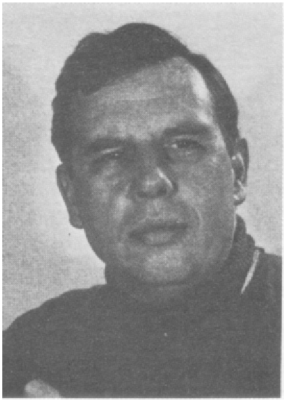

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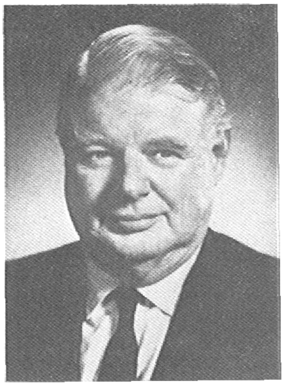

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